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OCTOBER 195

Zone Refining apparatus, showing tube and induction-heating coils. For transistors—tiny electronic amplifiers—germanium is made extremely pure. Then special impurities are added in controlled amounts for best transistor performance.

1 part in 10,000,000,000

To make the most of their revolutionary invention, the transistor, Bell Laboratories scientists needed ultrapure germanium.

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THE SCIENTIFIC MONTHLY

VOL. 79 OCTOBER 1954 NO. 4

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(*Courtesy Department of History, U.S. National Museum, Smithsonian Institution, see page 252*)

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Science and Technology

(From the month's news releases; publication here does not constitute endorsement.)

Lint-Free Absorbent Cloth

Although made especially for home consumption, this towel has a wide industrial use around delicate instruments in laboratories and in the manufacture of appliances and plastics that require lint-free, absorbent cloth. It is especially suited for wiping glassware, dishes, and windows and for general cleaning. Increased washing makes it more absorbent. (The Leshner Corp., Dept. SM, Hamilton, Ohio)

Fishing Kit

Included in the portable, heavy steel kit are a carborundum stone for sharpening hooks, knives, and so forth, a small rise for tying flies, a felt buffing wheel for polishing tackle, a small fiber brush for cleaning reels, and a new scaler that enables fishermen to scale fish faster than old methods. (American Homecraft Co., Dept. SM, 3714 Milwaukee Ave., Chicago 41, Ill.)

Venetian Blind Cleaner

A new vacuum cleaner attachment cleans venetian blinds where they hang, quickly and easily. Power-driven twin brushes are curved to fit the contour of the slats and clean top and bottom of slat simultaneously. Suction deposits the dust in the vacuum cleaner. (Pionair Products, Inc., Dept. SM, 603 W. Washington Blvd., Chicago 6, Ill.)

Unique Flashlight

A new flashlight, powered by two standard pencil-type batteries, slips onto the forehead like a pair of glasses and throws a beam of light directly on the point of interest. Because it leaves both hands free, it is especially handy around the car and on fishing and camping trips. With its compact spectacle-type frame, the plastic flashlight slips easily into pocket, purse, or glove compartment. (The Ozilam Co., 1513 Arapahoe St., Dept. SM, Room 426, Denver 2, Colo.)

Lawn Edger

This tool trims along sidewalks and curbs with a steel blade that sharpens itself as it rubs against concrete curbs. A 50-in. handle lets the user stand while edging his lawn. (C. A. Regele, 408 N. Market St., Dept. SM, Lancaster, Pa.)

Sponge Dishwasher

Pour a little liquid soap or detergent into the hollow plastic handle and the sturdy sponge makes suds as it washes. It is especially suitable for washing glassware and for getting into hard-to-reach places. (Cossman Co., Dept. SM, 6612 Sunset Blvd., Hollywood 28, Calif.)

Spray Gun

Attached to a garden hose, the spray gun mixes soap or detergent with water to supply a spray of rich suds for washing cars, window screens, and storm sashes. The unit has a three-speed mixture control and can be used to apply soluble fertilizers, soil conditioners, weed killers, and insecticides to lawn or garden. (Nutritional Concentrates, Inc., Dept. SM, New Lexington, Ohio)

Traveler Tool Kit

Slightly larger than a cigarette pack, the traveler tool kit contains a hammer, chisel, saw, file, drill, punch, and screwdriver that snap on a special handle. The tools and small jack knife are sheathed in a zippered cowhide case and are manufactured in Germany. (Abbeon Supply Co., Dept. SM, 179–27 Jamaica Ave., Jamaica 32, N.Y.)

Heat-Repellent Paint

The heat-repellent paint, applied to a building's roof, reportedly may lower interior temperatures. This "air-conditioning" paint reflects heat, light, and infrared waves from its white surface. It has been applied successfully to steel, wood, aluminum, glass, asphalt shingle, and masonry. (Coating Laboratories, Inc., Dept. SM, Tulsa, Okla.)

Homemaster Six-Inch Saw

This handy tool (Fig. 1) will cut 2-in. dressed lumber at 90 degrees and is equipped with both bevel and depth adjustments. In addition, it handles the same type of work as a heavy-duty saw and has all the necessary safety features to protect the operator and his work, including an exclusive clutch that eliminates any possibility of kickback. (Porter-Cable Machine Co., Dept. SM, Syracuse 8, N.Y.)

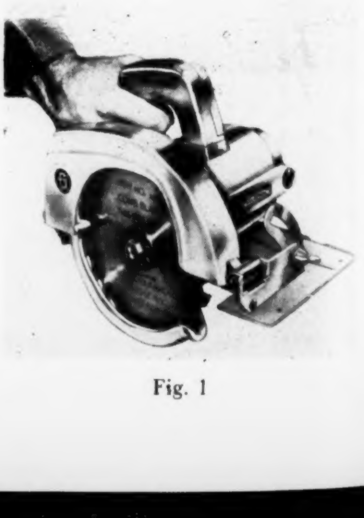

Fig. 1

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Superficial Aspects of Modern Organic Reefs*

PRESTON E. CLOUD, JR.

Dr. Cloud has been a member of the U.S. Geological Survey since 1941, except for 2 years that he spent on the faculty of Harvard University. His primary interest in paleontology and stratigraphy has been supplemented since 1948 by studies of modern organic reefs and lagoonal sedimentation in several parts of the Pacific Ocean.

RECENT announcement of a deep volcanic foundation beneath shallow-water Tertiary sediments at Eniwetok Atoll (*1, 2*) confirms the central assumption of the Darwinian view of reef evolution—great subsidence of atoll foundations (*3–7, 40.*) In the presently developing hypothesis of complex origin of reef types (for example, *40*, p. 453; *9*), therefore, previous alternative hypotheses may now be considered in their proper perspective as elaborations of the originally oversimplified subsidence theory (*10*, pp. 501–518; *11*). But reefs have surfaces as well as foundations, and a complete hypothesis of reef evolution should include an explanation for their surficial features as well as for their broad structural variety.

For example, some contemporary reef flats are areas of flourishing organic growth, while others are relatively barren of conventional reef-building organisms. Although the surf-facing fronts of many reefs display a radial comb-tooth pattern of alternating spurs or buttresses and intervening narrow grooves, other reef fronts lack such a toothed edge or have only an irregularly lobate or papillate front. The outer edges of some existing reefs are defined by a low algal ridge, which is missing on others. Some barrier reefs and atolls support islands of calcium carbonate sand and gravel of organic origin, but other reef surfaces lack islands.

It is the thesis of this paper that these superficial similarities and differences between modern organic reefs may be susceptible to a general explanation, from which various elaborations are possible. The proposed explanation calls upon the effects of a world-wide 5- to 6-ft fall of sea level that began about 3000 (± 1500) years ago, coupled with slight recovery from the lowest point of fall, beginning a century or so ago.

Recent Eustatic Variations of Sea Level

From the time of J. D. Dana (*12*) geologists have periodically noted evidences for recent slight changes of ocean level, and there are now many references to this subject. During the 1920's Daly (*13; 14*, pp. 174–179) emphasized a world-wide 6-m fall and suggested that it might correlate with a withdrawal of oceanic water during late growth of

the Antarctic ice cap "about 3500 years ago" (*14*, p. 179). Later Kuenen (*8*, pp. 66–70) concluded that the Indonesian region provided evidence of three recent drops in sea level, one from 4 to 5 m above present sea level, a second from 1.5 to 2 m, and a third from 0.5 to 1 m. Sewell (*15*, pp. 464–478) and Gardiner (*16*, pp. 34–39) noted a 5- to 10-ft fall of sea level in the Indian Ocean. From wide experience in the Pacific, Stearns (*17*, p. 780) expressed the view that a "5-foot recession . . . may have occurred only about 5000 years ago." He has also summarized data related to this and other possible late Quaternary eustatic shorelines (*18*). Recently emerged strand lines are also recorded in Australia from 2, 5 to 6, and 10 to 11 ft (*19–21; 22*, Fig. 38; *23*, pp. 75–77).

Observations by the authors named and others from many parts of the world indicate various recent relative changes of sea level, both positive and negative. The problem is to establish whether these changes are due to local causes or denote events of world-wide significance, such as changes in the total volume of ocean waters or the basins that contain them. In extratropical latitudes, the situation is complicated by postglacial isostatic rebound, resulting from unloading of continental glaciers, as well as by the gravitative attraction of sea water toward the heaped-up ice during glaciation (*24*, pp. 299–300). Purely local relative changes of sea level at many localities are attributable to tectonic causes—to local movements of the land itself, such as the upward movement of parts of the Sagami Sea coast of Honshu as a result of the 1923 earthquake. Evidence for the most recent significant fall of sea level, however, is now known to be so widespread and of so nearly uniform degree that no reasonable doubt remains that a change in the level of the sea itself is involved and not merely a coincidence of relative changes—a true eustatic fall of sea level. Since local evidence indicates this to be of the order of 5 to 8 ft, and most generally 5 to 6 ft, it will here be called the 6-ft or 2-m fall. It might as well be called the 5-ft fall, and will occasionally be referred to as the 5- to 6-ft fall to keep this in mind. As long as this is understood, though, no harm will come from the general use of a shorthand approximation that converts easily to a whole figure in meters.

Evidence for this particular fall of sea level consists of (i) solution notches in limestone cliffs and "mushroom rocks" about 5 to 6 ft above a notch that apexes near present high-tide level (Figs. 1*A*, 1*B*); (ii) wave-eroded benches (Fig. 1*C*) and solution benches (*25*) between 5 and 8 ft above the level of similar benches of the present sea; (iii) elevated beach rock (Figs. 1*A*, 1*D*), reef surfaces (Figs. 1*D*, 1*E*) and reef-flat rubble trains (*26*, p. 59); and (iv) low-level marine terrace deposits and strand lines (*27*, p. 104).

A temporary halt or slowdown in the rate of withdrawal of water at roughly 2 ft above present sea level is also widely evident, but this is regarded as an incident in the 6-ft fall. The effects of Pleistocene sea-level changes (*24, 28*) are mainly preliminary to those of the more recent 6-ft fall insofar as the superficial aspects of reefs are concerned. These effects prepared the reefs of early Recent time to be affected by the later 6-ft eustatic fall.

Although the beginning of the 6-ft fall of sea level cannot yet be conclusively dated, the tentative consensus of qualified radiocarbon analyses, anthropological and historical conclusions, and geologic inference suggests a date of about 3000 (± 1500) years ago (*19, 29–33*). The probable cause of sea-level recession involves withdrawal of water from the oceans to form additions to the present ice caps. Extension of this hypothesis relates the beginning of the 6-ft fall to the most recent subpeak of warmth in the postglacial interval of maximum warmth and dryness called the postglacial thermal maximum, or the altithermal.

General increase in world temperatures, beginning a century or so ago and accelerated in recent years, appears to have at least temporarily reversed the process and initiated some variations in the effects of the original sea-level fall (*19; 21; 22*, p. 111; *31*, pp. 17–18; *34*). The present rate of rise in sea level is variously calculated at about 2.5 to 8 in. (*35*, pp. 721, 730; *36–37; 38*, p. 80) to even as much as 2 ft (*39*, p. 275) per century. If this continues, the sea will be back to its former level in another 900 to 3000 years, or possibly in as little as 300 years.

General Features of the Upper Reef Surface

It is safe to assume that when the 6-ft fall began the upper surfaces of some existing reefs were within 6 ft of sea level, others were nearly 6 ft below sea level, and still others were at depths significantly more than 6 ft below sea level. This exercised an important effect on the superficial characteristics of present reef surfaces.

In tectonically quiescent areas only the reef sites that lay significantly below the 1-fathom level at the time of initiation of the 6-ft fall can today maintain upper surfaces of flourishing coral growth except at beveled and now recovering margins (Figs. 2*A*, 2*B*). Most reefs or parts of reefs that extended to within 6 ft of sea level when the fall

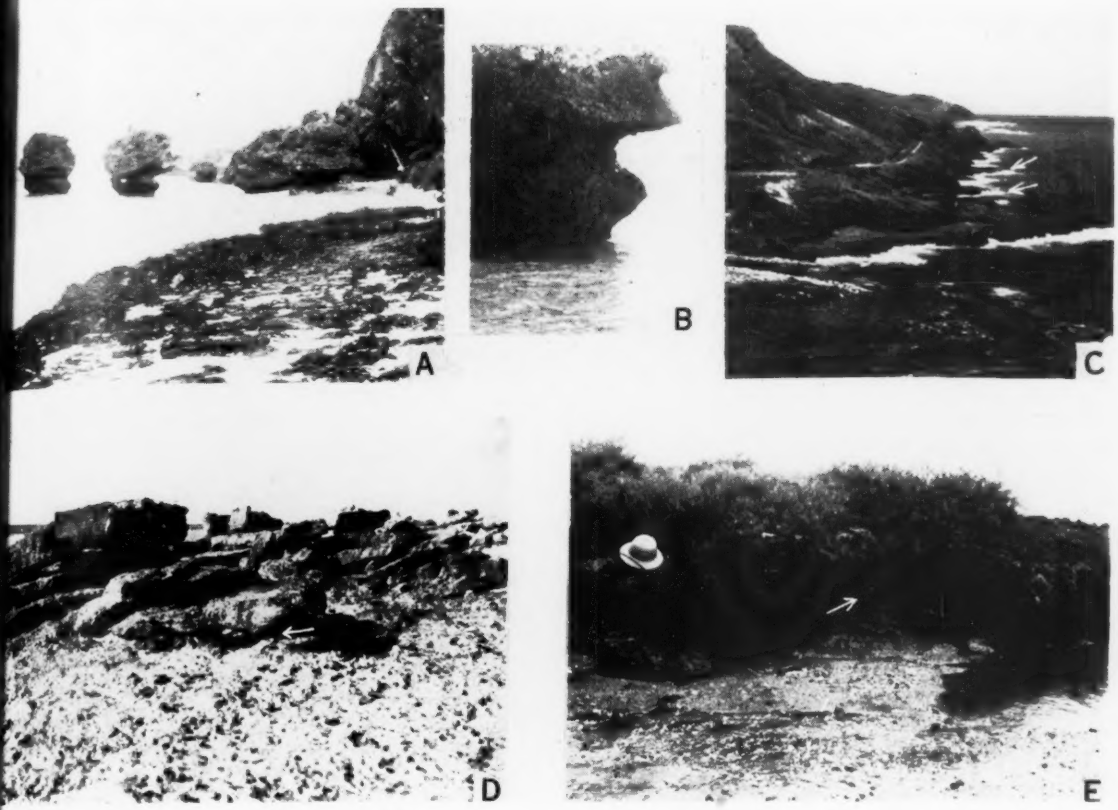

Fig. 1. Evidences of 5- to 6-ft fall of sea level. (*A*) Notches of present and 5- to 6-ft sea surrounding mushroom rocks in northwest Guam, Mariana Is. Tide is high. (*B*) Notches of present and 5- to 6-ft sea, southeast Saipan, Mariana Is. Tide is high. (*C*) Wave-eroded bench (arrows) of 5- to 6-ft sea east from Hanauma Bay, southeast Oahu, Hawaiian Is. (*D*) Elevated beach rock in contact with truncated old reef surface (arrow), Kwadak Islet, east margin Kwajalein Atoll, Marshall Is. Camera case is 8 in. high. (*E*) Elevated fringing reef veneering old basaltic rocks (arrow at contact) southwest Guam, Mariana Is. Tide is intermediate.

began have been truncated and smoothed to essentially flat surfaces (Figs. 2*C*, 2*D*) by solution, by the chemical and abrasive activities of organisms, and by the abrasive and other hydraulic effects of wave erosion. They are areas poor for growth of corals and crustose coralline algae, but they are inhabited by boring and crevice-dwelling organisms and are commonly veneered with drifting calcareous debris and soft algae or articulate corallines (Fig. 3*C*). Some, as in parts of the Gilbert Islands, slope gently seaward toward the reef edge and a peripheral algal ridge. Others are nearly or quite horizontal, perhaps because of accelerated solution effects such as might occur in areas of higher rainfall, or because of partial restoration by sheetlike algal veneers on the outer reef flat.

It is to be expected that nontruncated reefs will be found in areas of truncation, for it is unlikely that all reefs of a given area or all parts of a given reef reached uniformly shoal depths before the 6-ft fall. Likewise, what depth would be significantly below the original 6-ft level depends on the growth habits and growth rates of the reef organisms involved in building the particular reef (*40*, pp. 420–421; *41*, p. 2130). Local subsidence may have caused some recently emerged surfaces to be again flooded by the sea and to support renewed coral growth. Still other reefs that were formerly flourishing will have grown into the zone of truncation since the 6-ft fall began. Reefs of this type may be represented by the generally flat reef surfaces that rise exactly to, or only slightly above, mean low-tide level and display numerous unfilled holes and irregular areas or pockets of relatively loose interstitial limesand. These stand in contrast to the generally solid and sloping rock platforms of the elevated and then truncated reef flats.

Although described reefs differ greatly in detail, all that rise to or near sea level display generally similar gross patterns of construction and

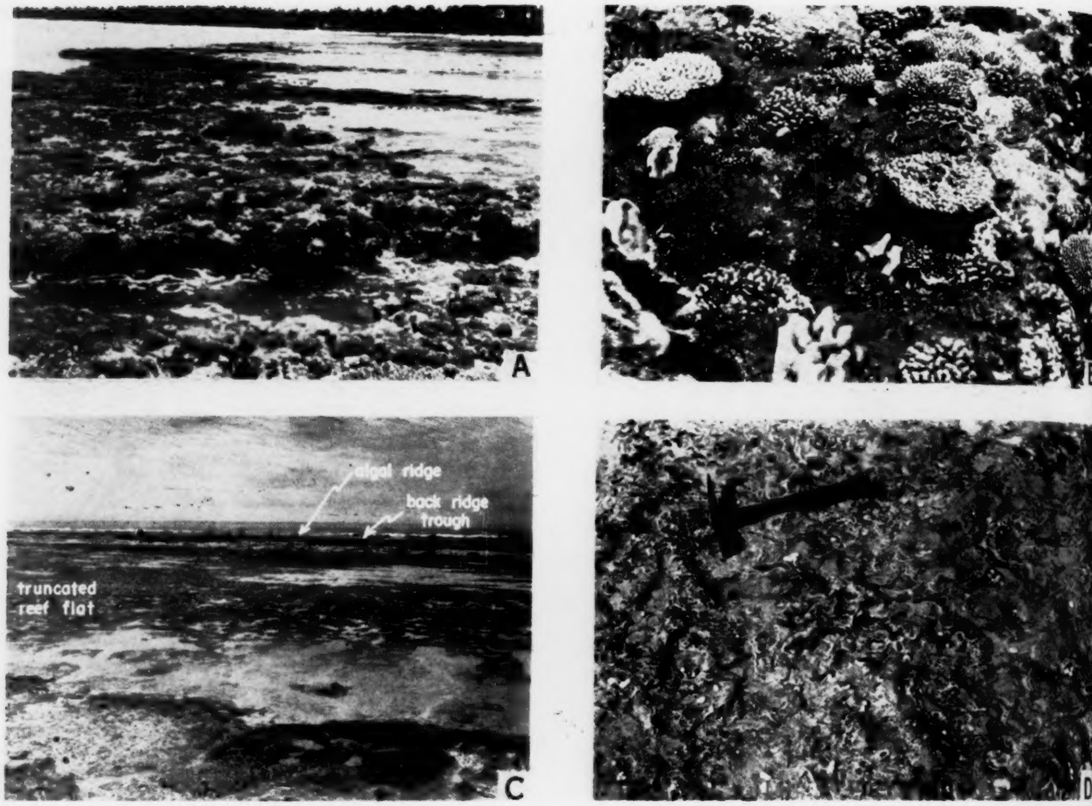

Fig. 2. Reef flats of rich organic growth and of truncation. (*A*) Rich coral growth along reef edge opposite I[illegible] village, south side of Arno Atoll, Marshall Is. Tide is low. (*B*) Close view at same locality. (*C*) Truncated reef flat opposite Government Station, east side Onotoa Atoll, Gilbert Is. Tide is low. (*D*) Close view of inner edge of reef flat near *C*. The irregular loops are truncated branches of the blue coral *Heliopora* in position of former growth.

erosion, both on and adjacent to the reef flat proper. As is emphasized by Ladd and others (*42*), either these patterns are parallel to the reef edge but random within the parallel zones, or they consist of parallel bands of features that individually are normal to the reef edge. The first type will be called concentric and the second radial. Concentric features are represented by the marginal algal ridge and back-ridge trough (where present), by growth and sedimentation zones of the reef flat and of the lagoon and reef slope near it, and by many of the larger reef islands. Radial patterns include rilled rock beaches, tide-pool chains, reef-flat rubble trains, beach cusps, the groove and spur systems at the toothed edge of the reef, and some reef islands. Because construction and erosion are near or in a state of balance on most surface-breaking reefs, it is not possible consistently to correlate either principal pattern type with dominance of constructional or erosional forces, but each form or group of forms must be studied as a separate problem.

The Reef Edge

At low tide a low but prominent purplish-red ridge (Figs. 3*A*–3*C*) may commonly be observed at the outer margin of surface-breaking reefs. This ridge seems to be constructed mainly of the crustose coralline alga *Porolithon* (Fig. 3*B*) and is therefore called the algal ridge (*Lithothamnion* ridge or coralline ridge in some reports). It is ordinarily intersected by narrow grooves or surge channels (Figs. 3*B*, 3*D*) except where they have been roofed over (Fig. 4*D*). Association of algal ridge, grooves, and islands or evidences of former islands may be usual and significant, as will be noted.

Water that washes over the algal ridge or through the surge channels on some reefs maintains a parallel, shallow, water-filled depression (Figs. 2*C*, 3*A*, 3*C*) behind the ridge, even at times of lowest tide. A sparse growth of coral is characteristic of this back-ridge trough where it is present, even on reef flats that elsewhere are barren of normal reef-building organisms.

Where present, the back-ridge trough may be terpreted as (but is not known to be) a remnant the erosionally beveled reef margin, walled off upgrowth of the algal ridge. Upgrowth of an gal ridge at the reef edge might well follow minution of abrasion as an eroding reef bench eared the new sea level. Wherever the back-dge trough is lacking either the reef margin may ot have been markedly beveled, or else the bev-led front may have been completely restored by rganic overgrowth over a wide area. The slight cent recovery of sea level already noted has prob-bly further stimulated growth of the algal ridge s well as slight recovery of coral growth in the ower parts of truncated reef flats.

The Reef Front and the Radial Groove and Spur System

Parts of the seaward margins of many living eefs display a comb-toothed pattern of alternating elongate spurs and intervening narrow grooves (Figs. 3*A*, 4*D*, 5*C*), the spurs commonly veneered by or consisting of luxuriantly growing crustose coralline algae. Around a high island, such as Saipan of the Mariana group, the toothed margin is likely to be well and regularly developed where surf is strong, the offshore slope not too precipitous, and the reef margin not more than about 1/3 mi from shore (Fig. 5*C*). Wherever waves spill freely into a lagoon with an ample outflow pass or passes, grooves are likely to be weakly developed and irregular or absent. Wherever crustose coralline algae grow vigorously at such locations, or wherever a land-backed reef flat is very broad or surf is weak, associated algal growth is likely to diverge from the regular comb-toothed pattern. Here it either tends to take the form of broad irregularly lobate algal spurs with narrow irregular interspaces or to effect a papillar pattern (Fig. 5*B*) of numerous closely spaced algal bosses or

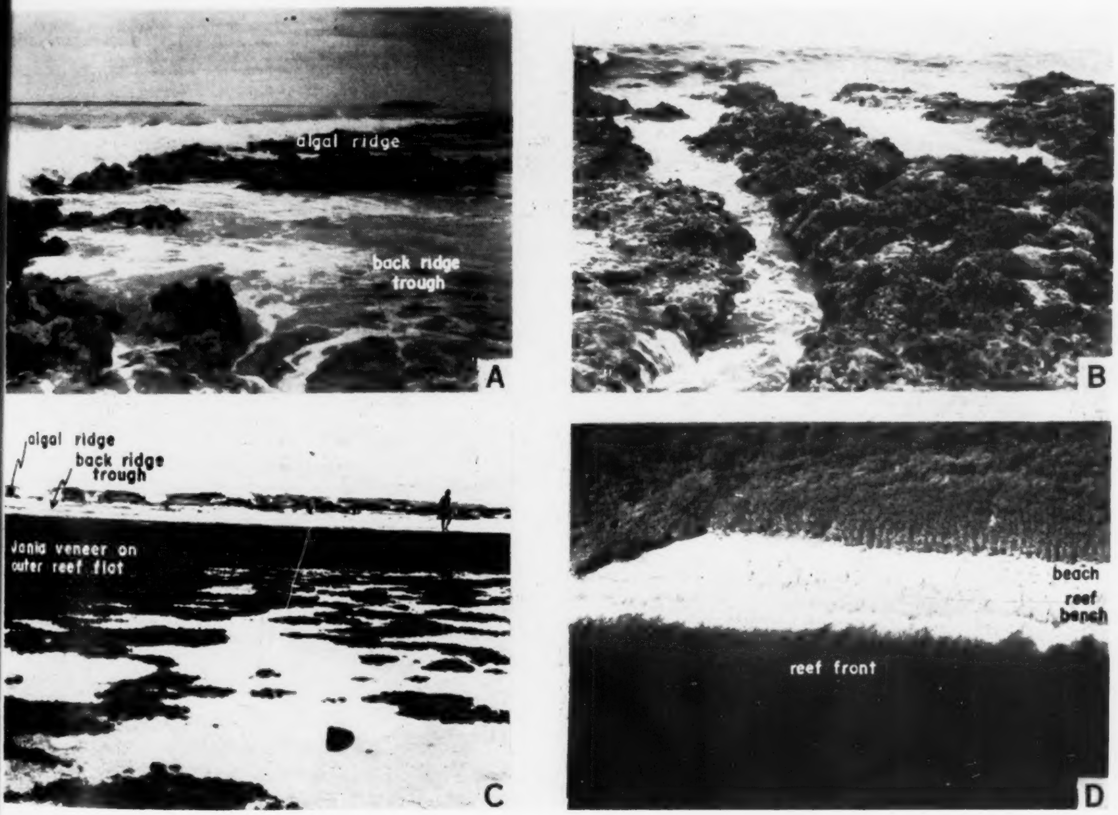

Fig. 3. Algal ridge, back-ridge trough, 6-ft bench. (*A*) Algal ridge of coralline alga *Porolithon*, east side of Onotoa Atoll, Gilbert Is. Tide is low. (*B*) Close view along surge channels and algae-veneered upper spur surfaces at same locality. (*C*) Back-ridge trough between figure and algal ridge at wave front; the articulate coralline alga *Jania* veneers bench surface in foreground. Same locality. (*D*) Oblique airview of groove and spur system at front of partially eroded reef bench that was relatively elevated by 6-ft fall of sea. East end Tarague Beach, North Guam, Mariana Is.

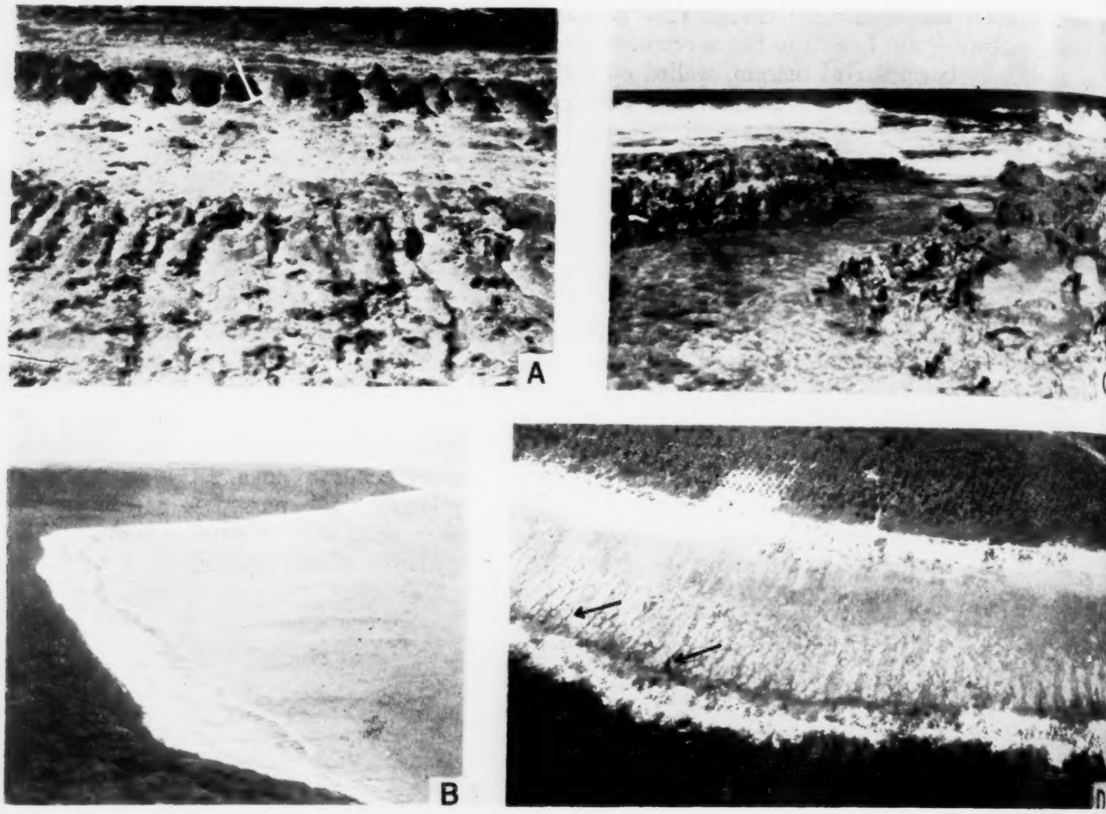

Fig. 4. Rilled rock beach, groove and spur systems, surge channels. (*A*) Radially rilled intertidal rock beach on lagoon shore at Onotoa Atoll, Gilbert Is. Tide is low. Hammer is 15 in. long. (*B*) Oblique airview along Tarague Beach, North Guam, Mariana Is. Dark strips of elevated reef rock separate water in reef-flat grooves that continue seaward across present reef edge. (*C*) View seaward along outer part of reef-flat groove at near end of *B*. Elevated reef rock of 6-ft sea at sides of groove shows jagged solution surface. Tide near high. (*D*) Groove and spur system with conspicuous organic overgrowth roofing inner parts of grooves and forming under-reef caverns (arrows) at west end of Tarague Beach area, North Guam, Mariana Is.

knolls. Such algal bosses may occur in seemingly random arrangement but more commonly tend to line up and grow together as algal spurs (Fig. 5*B*). The algal buttresses or spurs seem to be broader and more irregular wherever the reef is very narrow, the offshore slope steep, or the surf light. The grooves also are probably more irregular on steep than on moderate to gentle offshore slopes. Groove systems, however, are commonly somewhat irregular and may fork either seaward or shoreward. Their most nearly invariable characteristic is an arrangement approximately normal to the reef and wave front.

On windward sea-facing reefs with moderately sloping fronts, and opposite islands of atolls and barrier reefs, the groove and spur systems appear to be ordinarily of the typical comb-tooth pattern. On leeward or protected sea-facing parts of the same reef types the spur systems tend to be irregular or to give way to papillar patterns of algal growth. Along the shores of high islands the grooves at places may be related to joint systems in the rocks and may trend obliquely to the reef edge.

The broad and irregularly lobate algal spurs and the papillar patterns (Fig. 5*B*) seem most satisfactorily explained as growth features. Other patterns of radial grooves and spurs at the fronts of shoal or sea-level benches are probably best accounted for by erosion. Examples of groove systems of demonstrably erosional origin are those that occur seaward from limestone cays in oolitic underwater bench deposits of the Bahama Islands (*43*, p. 25) and in the margin of a sea-level bench in volcanic conglomerate in eastern Saipan (Fig. 5*D*). Between the extremes indicated there is probably a complete spectrum of genetic kinds of groove and spur systems—from mainly erosional to mainly constructional—and of later modifications of the two end-types by both erosion and organic growth. Hoffmeister (*44*, p. 9) was an early ad-

ocate of an erosional origin for the toothed edge f normal Pacific reefs, but recent reviewers (*40*, . 431; *42*, p. 413; *45*) have emphasized the importance of growth factors. I will undertake here o develop the place of erosion in the formation f groove systems and to relate this to the 6-ft fall f sea level. It should be understood, of course, that he balance of factors involved in the formation of ny particular groove and spur system can be determined only locally.

Erosional Factors in Evolution of Groove and Spur Systems

Hydraulic erosion (Fig. 6) is believed to be of leading importance in the origin of the markedly regular groove and spur systems of many nearshore reef fronts. The inferred mechanism involves the erosional origin of a basic pattern of subradially arranged residual spurs that subsequently became veneered and extended, or even obliterated, by algal growth.

Special significance is attached to abrasion during return flow seaward from elevated reef benches of surf-driven waters, to intertidal solution, to the formation of solution rills by fresh water in early stages of development or during preceding Pleistocene emergence, and to inhibition of organic growth in the grooves in late stages of development owing to continuing outward movement of a diminishing sediment train.

Underwater observations of dye flow and drift of detritus in transit in grooved areas show a con-

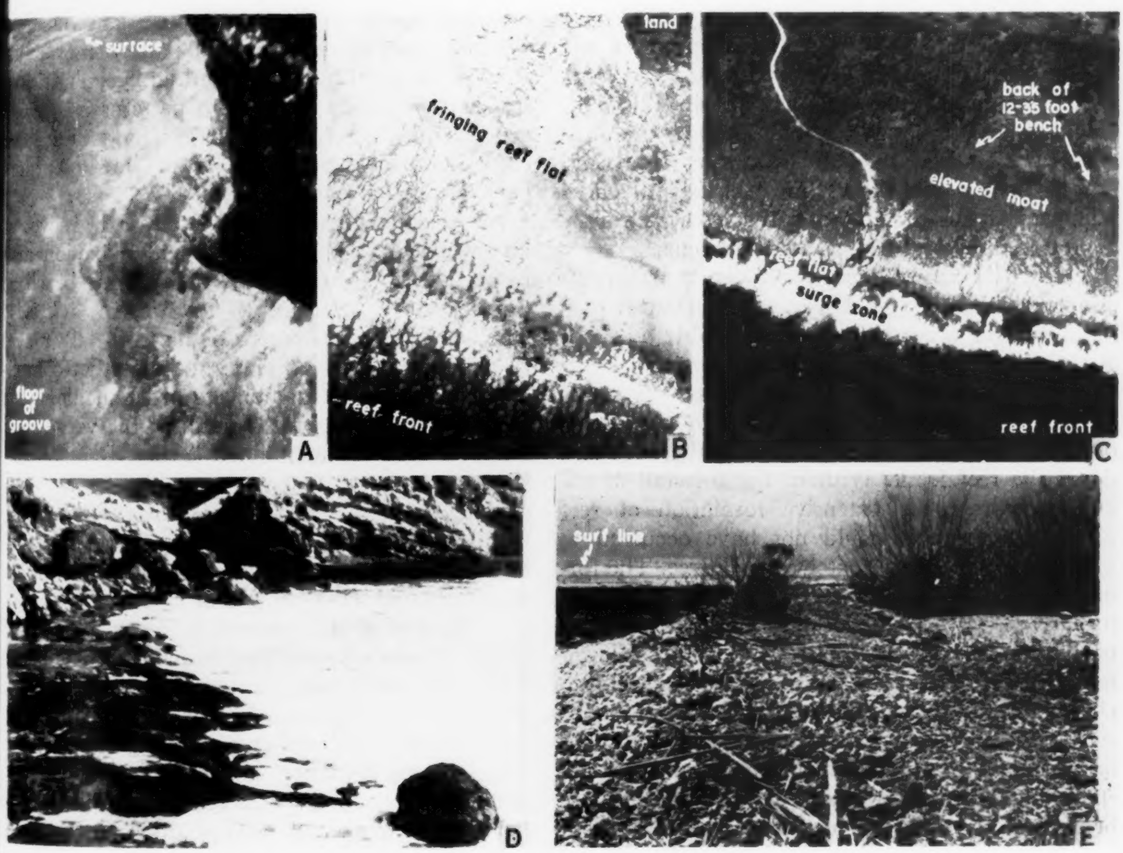

Fig. 5. Groove and spur systems, elevated grooves, gravel rampart. (*A*) Undercut side of erosional groove on elevated reef bench at east end of Tarague Beach, North Guam. Same groove as Fig. 4*C* farther toward shore. Tide is near high, water about 3 to 3½ ft deep. (*B*) Oblique airview of papillar algal growth grading to a spur system at front of fringing reef along the east coast of Guam, Mariana Is. (*C*) Groove systems of 15- to 40-ft sea (12- to 35-ft bench) and of present sea along north shore of Saipan. Elevated supposedly erosional (cuts older rock) groove system between present narrow fringing reef and elevated moat gives the impression of being essentially continuous with that of present reef front. (*D*) Surge-channel zone of groove and spur system in Eocene volcanic conglomerate at Hagman Beach, east Saipan. (*E*) Gravel rampart at northeast end of Onotoa Atoll, South Gilbert Is. To right of gravel ridge is remnant of elevated, radially gravel-striped reef surface, to left is somewhat less elevated old reef-flat conglomerate, and beyond is truncated modern reef-flat.

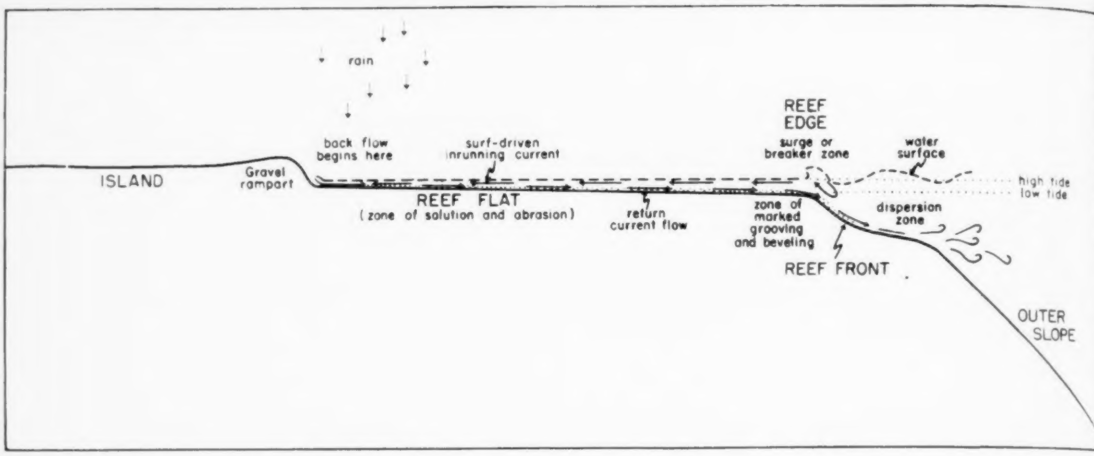

Fig. 6. Some hydraulic factors and zones of activity on an eroding reef surface.

tinuous outflow seaward of a basal layer of water across the reef flat and down the uppermost reef slopes, interrupted only by the back-and-forth surging of the breaker zone (*26,* pp. 41–42). This would appear to be greatly intensified by the presence of islands at moderate distances from and parallel to the reef fronts, for such islands cause piling up of surf-driven water that escapes mainly by return flow seaward as undercurrents.

This basal return flow, wherever it transports clastic calcium carbonate or other sedimentary particles by saltation or in suspension, could and probably does scour the reef flat. The flow would presumably be channeled to some extent by any initial irregularities on this surface or at the outer edge of the reef or bench as it pours over the edge and down the reef front. Without the 6-ft fall of sea level and resulting extensive formation of reef islands, it probably could not have occurred to anything like its recent extent and degree. The formation of solution rills on slightly elevated benches in early stages of development, inherited patterns of solution rills or residual irregularities from lower sea level stands, or even joint patterns may help to channel abrasive materials and solvent waters, as suggested. In the course of centuries, radial systems of subequally spaced erosional grooves and residual spurs might thereby evolve, extending to the more or less uniform depth of effective undercurrent action but being most pronounced in their upper parts. Many of the patterns seen are reminiscent of those produced by outflow of sand-bearing and calcium carbonate-dissolving intertidal water across rock beaches (Fig. 4*A*) or of solution rills on sloping limestone surfaces. Spacing of spurs is comparable to beach cusps, reef-flat rubble trains, and some solution-abrasion rill patterns on rock beaches (for example, the southeast side Cocos Island, Guam). Modifications of the erosional pattern may result from adjustment not only to initial irregularities of bench morphology but also to patterns of organic growth outward from the reef front, and some elaboration by accompanying or subsequent organic growth is characteristic.

An emerged bench crossed by radial grooves at an intermediate point in erosional reduction may be seen in the eastern part of Tarague beach at the north end of Guam (Figs. 3*D*, 4*B*, 4*C*, 5*A*). The retarded development of the reef-flat grooves at this place, however, seems to be further complicated by renewed organic construction along the reef margin, perhaps as a result of the very recent sea-level recovery mentioned earlier. Remnants of slightly emerged surfaces elsewhere (*25*) occasionally point to the erosional origin of other existing reef surfaces and probable erosional control of the regular groove systems at their margins. Both elevated (Fig. 5*C*) and submerged tiers of grooves, presumably related to older intervals of bench truncation and reef growth and at places coinciding with joint patterns, occur on Saipan and probably other Mariana Islands.

As the elevated reef or bench is gradually worn down, lateral planation and the action of hydraulic pressure in confined spaces reduces the biologically bored and weakened, and solution-pitted, intergroove remnants (Fig. 4*C*) to the general level of the existing sea. Eventually the amount of detritus in transit is lessened, and both abrasion and solution effects decrease in importance with progressive lowering of the emerged surface. Coralline algae and corals reestablish themselves in the upper parts of the walls and fronts of the reef-front spurs as the grooves are lowered; and accel-

ion of growth with reduction in the amount of ritus in transit and slight rise of sea level may d to roofing over of the grooves (Fig. 4*D*). In manner the traces of erosion may be obscured obliterated, the spurs extended seaward as with features (Fig. 4*D*?), and the regularity of pattern distorted. Under-reef caverns (Fig. 4*D*) blowholes commonly result from such overwth.

nless previously formed at lower water levels emphasized by intervening organic growth, oves are likely to be indistinct or absent across r-surface underwater benches, where the out- current is dissipated and dispersed (Fig. 6). example is the weakly grooved bench that de- nds from 2 to 10 fathoms in a distance of 180 ft so off the windward reef of Onotoa Atoll, in the lbert Islands. Here the grooves are barely per- ptible from the breaker zone outward across the per part of a strongly inclined bench that sup- rts sparse coral growth and is everywhere lit- ed with dead fragments of branching coral. The al litter is broken up nearly in place, the grooves de out seaward, and this part of the groove sys- n is probably the result of current sweeping (or clastic veneering of a Pleistocene groove system) her than of recently active erosion or growth. t other places growth of algal spurs may produce nilar grooves as residual features; perhaps con- olled with regard to location by continuing slow ward movement of sediment trains that may t erode but also do not provide a suitable sub- ate for growth of calcareous algae or corals.

The parts of the radial grooves that cross the eaker zone are called surge channels. In the lbert and Mariana Islands some algally veneered urs (Figs. 3*A*, 3*B*) that superficially are con- cing growth forms were found, by diving with e mask and aqualung, to be separated by ooves that undercut the spur bases (similar to g. 5*A*) and are floored with coarse gravel. Be- ath the surge zone, the intervening spurs them- lves are commonly of eroded appearance and ve only sparse organic growth. Some of the ooves off southwestern Saipan end in, or are in- rupted by, submarine potholes that contain arse gravel. Stearns (*46*, p. 787, Fig. 3) has also served cobble gravel in the reef-front grooves of niwetok, in the Marshall Islands; and I have seen avel and sand in motion in grooves off Cocos land in south Guam.

Gravel observed in grooves (not potholes) at notoa (Gilbert Islands) was mainly slabby and cluded pieces up to an estimated 4 ft across and to 10 in. thick—pothole gravel is generally smaller and subspherical or subellipsoidal. The largest slab seen to move at times of moderate surf was about 8 in. in diameter and 2 in. thick, but at times of great storm even the large pieces are probably moved, or they would not be worn as smooth as they are on all sides. Back-and-forth sliding in the zone of concentration in the surge channels may be a factor in shaping those gravel fragments that did not inherit their slabby form. The free diver suspended in the lower parts of the surge channels at times of even moderate surf finds himself carried back and forth 15 to 20 ft at a time as the waves break and then retreat. The probable back-and-forth movement of the gravel at times of strong surf may scour the grooves in the surge zone and produce oversteepened inner ends—or similar oversteepening may result from organic upbuilding of the outer reef flat in late stages of development.

Relationships between Grooves, Islands, Algal Ridge, and Wind Direction

In the Pacific Ocean area, at least, the grooves appear to be most abundant on windward reefs. However, they occur in all quarters of the wind and are believed to be directly related to something other than wind. Apart from slight emergence, the controlling factor in a well-developed regular groove system may be the presence of an island (or other barrier to water movement) at the inboard edge of the reef-flat. The surf-driven water piles up against this barrier and escapes by flowing seaward beneath the inrunning current. This detritus-bearing and, at times, calcium carbonate-dissolving undercurrent cuts the grooves or deepens previously established tracks. At the same time the reef margin is prepared, by beveling, for later upgrowth of the algal ridge—a growth form of clean, turbulent waters. Wherever a barrier is missing the water washes across into the lagoon behind (only weakly resisted by the lagoon waters themselves), return current flow is weak, abrasive materials are not likely to be abundant, and grooves are likely to be only weakly and irregularly developed. Greater frequency of grooves (and algal ridges) to windward is thus inferred to be related to greater frequency of islands to windward. The concentration of islands to windward presumably results from more numerous storms of average (rampart building) intensity and fewer destructive cyclonic storms from the normally windward direction.

Wherever grooves and surge channels are well developed without an immediately inboard island the former presence of an island, or other barrier

to cross-reef water movement, is suggested, and evidence of the missing island is to be looked for. Actually, if most of a reef-flat were planed as a result of the 6-ft fall, most of it may at one time have been an island or barrier to cross-reef movement of water. But more satisfying evidence of former land of some duration may be found. The bedded and indurated shore-line detritus known as beach rock, for instance, is conclusive evidence of former land. It also suggests the approximate position of such land, for, with important exceptions, it dips 4° to 7° lagoonward or 7° to 10° seaward from the land at its place and time of formation. Well or grave holes and native historical records are other sources of information on the presence and location of former islands. Such findings are important, for a test of the suggested hypothesis of groove origin might well be formulated around the frequency of a relationship between well-developed groove systems and algal ridges of the present sea level to islands or evidences of former islands at the inboard side of the reef-flat. In this connection it would be of interest to know more about the distribution of groove systems and algal ridges in Indian Ocean atolls, where it is said that most reef islands are found to leeward (*40,* p. 437).

Anomalies in Groove Distribution

Absence or inconspicuousness of the radial groove and spur topography away from areas of organic growth is a difficulty with the suggested importance of erosion in the formation of groove systems. However, this may be related (i) to the general absence from such areas of abruptly downsloping margins that would focus the action of undercurrents on the rims and outer slopes of relatively narrow near-surface or slightly elevated benches, or (ii) to the even greater rarity of soluble limestone benches that might accelerate or control grooving by solution rilling. Where intertidal or very shoal benches along oceanic coasts have abruptly descending fronts, have widths comparable to those of grooved reef-flats, and are backed by land, groove systems are to be expected. Those in oolitic limestones of the Bahama Islands and in a conglomerate bench in eastern Saipan (Fig. 5B) were noted in a foregoing paragraph. It is, of course, to be expected that tectonic effects and isostatic adjustments in unstable areas may also restrict the occurrence of well-defined radial groove systems through depression or through elevation too recent for erosion to produce noticeable effects.

Reef Islands

Reef islands are the islands of calcium ca bonate or "coral" sand and gravel that rise ord narily not much more than 10 to 12 ft and rar more than 18 or 20 ft above sea level from th surfaces of barrier reefs, the outer reefs of atol or isolated table reefs and patch reefs. The fou dations on which they rest for the most part prob ably consist of a reef that has grown to the su face of the sea, or one that, having grown to with 6 ft of the surface, was relatively elevated by th 6-ft fall of sea level. A few are based on old surfaces and, presumably, are at least partial rem nants of older islands (Fig. 1*D*). Without consid ering the relative adequacy of the two gener types of foundation, it seems self-evident that th 6-ft fall would have provided many foundation of the second type, as first suggested by Gardine (*15,* p. 464; *16,* p. 36). The existence of suc emergent foundations has been demonstrated i the Gilbert and Ellice Islands, and in some of th atolls of the Marshall Islands and Indian Ocea (*26,* pp. 47, 52).

Such surfaces would be susceptible to solutio by rain water and also to a certain amount o truncation by overriding storm waves. At the sam time, rubble tossed upon them would have a rela tively good chance of remaining in position at a distance from the reef front that would vary wit altitude, slope, and the transporting power o storm waves. It would not be surprising if, o such a surface, some great storm or series of storm eventually constructed a gravel ridge (Fig. 5E (*42,* pl. 6, Fig. C) or series of ridges that remained relatively stable along a given line and under pre vailing strength of waves. Ridges of this natur occur along the seaward front of many linear atol islets, such as those of Onotoa Atoll in the Gilber Islands (*26,* Figs. 2–3). Locally they are obscured by later dune sands and added to by linear seaward gravel accretions (*42,* pl. 6, Fig. C) that at places produce a composite series of ridges. Intertida cementation should help to anchor such a grave ridge. As solid cementation of a reef surface ap pears to be strongly favored by intertidal ex posure, old reef surfaces beneath islands may no be solid where the island debris began to accumu late and bury them before or very shortly afte full emergence occurred.

As a result of lagoonal longshore drift (caused by current sweeping around the ends of the ridge plus the effects of wind action and occasional storms, sands and fine gravel may accumulate be hind gravel ridges, once the latter are formed. Great storms may pile more gravel against th

ard edge of such a protective rampart, breach or move it further across the reef. Dune sands umulate over and behind it. Continuing drift und both ends, and storm wash from the lagoon , may form a broad low ridge or succession of es behind and lagoonward from the gravel e or rampart. Then follow further additions to lagoon side by still-continuing longshore drift, e wash, and dune formation. Windblown and nwashed debris in time comes to veneer, but ly to fill, the resultant medial depression. Remts of a medial depression are, in fact, generally ognizable even in late stages of island developnt and at places are the site of brackish- or h-water ponds.

Stages in island growth, according to the scheme lined, may be illustrated by the longitudinally red island strips of Marakei Atoll in the Gilbert ands (*47*, pls. 149 and 150), and by filling ce 1900 of lakes in the central depression of tali Island on Addu Atoll in the Maldives (*48*, 77). Sewell also deduces (*48*, p. 77) by reference pumice lines, that "the inner beach of the island s advanced toward the lagoon by some 10 yards" tween about 1885 and 1934. Beach-ridges, show- successive lagoonward growth, are commonly ible on aerial photographs of reef islands.

Owing to the well-known monsoon-trade wind le, storms are commoner from the normally ndward direction, and gravel ramparts and ands are more likely to form on windward reefs. e infrequent violently destructive storms, hower, seem most generally to come from the norlly leeward side and are prone to sweep potenlly island-forming debris or existing islands from e leeward reefs as they drive great waves across em. Thus the two factors may combine to favor and development and preservation on windward efs. At the same time, current interference near sses and projections (or horns) and partial deuction of previously formed islands probably ad to many patterns of island formation and ape that differ widely from the idealized form scribed in the foregoing paragraphs.

In fact it seems likely that even linear reef islands places developed in other ways—but many can e explained adequately by the suggested mechnism, and evidence of its operation may be obured in others by later deposition, storm breachg, or variations in the history and pattern of evelopment.

Whether in the manner outlined (Fig. 7) or by her methods, one can visualize the origin or aclerated development of reef islands as a result the 6-ft fall of sea level. Soon thereafter the central Pacific was probably dotted with island stepping stones on which a distinctive vegetation took root and by way of which the Polynesians could have traveled within sailing distance of Hawaii. Indeed, it seems reasonable to take the present wide development of the habitable reef-island stepping stones as, in itself, presumptive evidence for the 6-ft fall of sea level.

Even today some of these islands are still growing through additions to their lagoon margins, and, as is shown by buried humus layers, through accumulation on their upper surfaces of the wind-borne tests of Foraminifera and other fine particles of calcium carbonate from the reef-flat. At the same time other islands are being eroded. Still others have been completely or partly washed away at times of great storm, if one accepts the evidence of beach-rock remnants and old well or grave holes (*10*, p. 512) on intertidal or shoal flats not adjacent to islands. Basically they are evanescent features. The destructive effects of storms will presumably increase as the sea level rises again, and, if the present rate of recovery continues, the sea will regain its former 6-ft level before a great many more hundreds of years. Under these conditions it is implicit that most of the reef islands will be washed away or reduced to uninhabitable remnants unless reef and island growth keeps pace with rising sea level. On the other hand, if the present warm climate is merely a temporary reversal of a major trend toward cooler climatic conditions and enlargement of glaciers and ice caps, existing islands may be further elevated and enlarged, truncation of presently flourishing reef surfaces will eventually ensue, and new islands will appear.

Summary

A general explanation is suggested for the superficial aspects of existing organic reefs. This involves the effects of a 5- to 6-ft fall of sea level that began about 3000 (± 1500) years ago, coupled with slight recovery from the lowest point of fall beginning 100 years or so ago. Evidence for the recent 5- to 6-ft sea level drop (called the 6-ft fall for brevity) is found in all tropic oceans. Its cause is seen in renewed growth of ice caps since the postglacial thermal maximum.

The surfaces of reefs that lay within 6 ft of sea level when the 6-ft fall began were subject to truncation by abrasion and solution. They ordinarily support few reef-building organisms today. Conversely, present sites of most flourishing reef surfaces were probably more than 6 ft below sea level when the 6-ft fall began. The low ridge of coralline algae that fringes the outer margin of many

reefs, and commonly walls off a back-ridge trough behind it, may have grown up from the beveled reef edge with decline of bench reduction and initiation of sea-level recovery. The latter factors also stimulate local renewal of coral and algal growth at other places, especially at reef margins. Tectonic influence locally alters the general scheme.

The toothed edge of radial grooves and spurs at reef fronts is attributed to the balanced interplay of erosion and growth. Both solution and abrasion are believed commonly to play a part in the radial pattern, especially during reduction of elevated reef surfaces such as resulted from the 6-ft fall of sea level. The preponderance of well-developed regular groove systems and algal ridges off windward reefs may be related to preponderance of reef islands (or former islands) to windward. Surf-driven water, barred by islands, streams se- ward in detritus-laden and commonly solvent (fr- rain dilution and lowered pH) return currents th- furrow and bevel the reef margin and later inhi- organic growth along established outflow paths. O- ganic overgrowth in the noneroding parts grooves and surge channels, along the beveled re- margin and seaward from eroded spur fron- later adds to, produces variations of, obscures, obliterates the primary erosional effects. Syste- of very broad irregular spurs and of conflue- algal bosses seem to be wholly constructional. Th- particular balance of factors that resulted in th- origin of any given groove and spur system therefore, an individual problem. Probably not parts of all groove and spur systems date from th- 6-ft fall of sea level alone, but the effects of th-

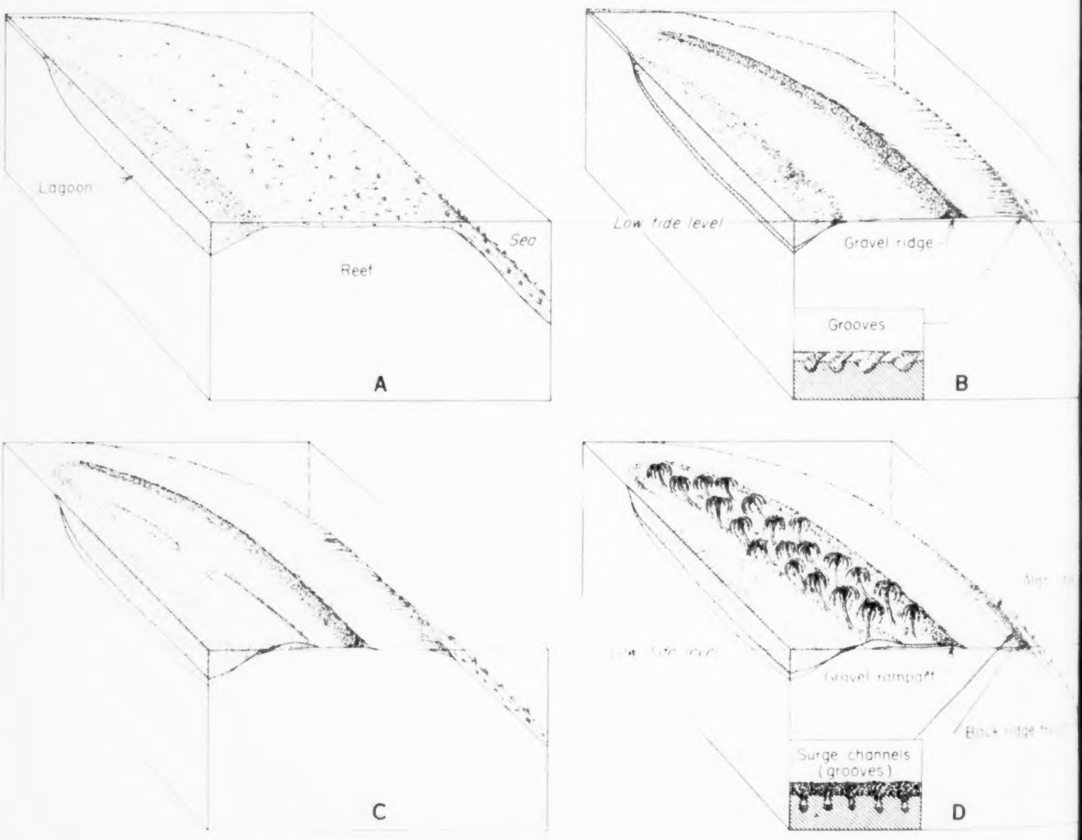

Fig. 7. Conjectural stages in the evolution of a slightly emerged reef surface (highly generalized and telescoped) (*A*) Reef surface within a fathom of sea level before beginning of 6-ft fall. (*B*) Six-foot fall well under way. Storm- built gravel ridge approximately stabilized on eroding reef bench. Beginning of groove erosion. (*C*) Six-foot fa- nearly complete. Bar formation results from current drift around ends of gravel ridge and storm wash. Effects bench reduction decreasing. (*D*) Six-foot fall completed and slight recovery of sea level under way. Island constru- tion by longshore drift, storm wash, and dune formation now complete and vegetation established. The gravel rid- is now a gravel rampart protecting the island behind it from erosion. Growth of algal ridge has walled off a ba- ridge trough at outer edge of reef flat. Parts of grooves through algal ridge are surge channels.

nt are believed to be widely significant for their gin.

he abundance of habitable low reef islands is attributed to the 6-ft eustatic fall, and an rred sequence of development for a concentric ar reef island is graphically summarized by 7. If present world temperature increase con- ues, however, the sea will be back to its former l in another 300 to 3000 years and such islands l be in danger of eradication by wave attack, ss reef and island growth keeps pace with ng sea level.

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The essential fact of significant subsidence is, of course, also supported by evidence such as the volcanic pinnacle in the rim of atoll-shaped Clipperton Island (*4*, p. 38; U.S.H.O. Chart 1680; *5*, p. 349, pl. 26), by paleontologic data from mid-Pacific seamounts (*6*), and by drilling and marine geology studies summarized by Shepard (*7*, pp. 669–670) and by Kuenen (*40*, pp. 445–458, 447).

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The Scientists Must Speak Up

The problem of the effects of H-bomb explosions is terribly disturbing, but I do not think that a conference of scientists is what is needed to deal with it. There are too many conferences in the world today and too many decisions taken by them.

What the world should do is to listen to the warnings of individual scientists who understand this terrible problem. That is what would impress people and give them understanding and make them realize the danger in which we find ourselves.

Just look at the influence Einstein has, because of the anguish he shows in face of the atomic bomb.

It must be the scientists, who comprehend thoroughly all the issues and the dangers involved, who speak to the world, as many as possible of them, all telling humanity the truth in speeches and articles.

If they all raised their voices, each one feeling himself impelled to tell the terrible truth, they would be listened to, for then humanity would understand that the issues were grave.

If you and Alexander Hadlow [who has pleaded for a United Nations conference of scientists on the H-bomb] can manage to persuade them to put before mankind the thoughts by which they themselves are obsessed, then there will be some hope of stopping these horrible explosions and of bringing pressure to bear on the men who govern.

But the scientists must speak up. Only they have the authority to state that we can no longer take on ourselves the responsibility for these experiments, only they can say it.

There you have my opinion. I give it to you with anguish in my heart, anguish which holds me from day to day.

With my best wishes and in the hope that those who must advise us will make themselves heard.—ALBERT SCHWEITZER.

This letter appeared originally in the London Daily Herald *and was reprinted in* Science *with Dr. Schweitzer's permission. It also was reproduced in the* Saturday Review *for 17 July at the suggestion of Eugene Exman, who is Dr. Schweitzer's American editor. Dr. Schweitzer currently is dividing his time between his home in Gunsbach, Alsace, France, and his hospital in Lambaréné, French Equatorial Africa.*

The Present State of Operationalism

This introduction and the six articles that follow—by Henry Margenau, Gustav Bergmann, Carl G. Hempel, R. B. Lindsay, P. W. Bridgman, Raymond J. Seeger, and Adolf Grünbaum—comprised a symposium on "The present state of operationalism" (or as some authors prefer to say, "of operationism") held in Boston, Massachusetts, in December 1953. The symposium was sponsored by the Institute for the Unity of Science, the National Science Foundation, and the American Academy of Arts and Sciences and was the second part of a five-part conference on the general subject Validation of Scientific Theories. *The papers comprising the first symposium, "Reasons for the acceptance of scientific theories," appeared in the September issue. The papers for the remaining three symposiums will appear in subsequent issues.*

On Interpretations and Misinterpretations of Operationalism

HENRY MARGENAU

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IN introducing this symposium on "The present state of operationalism," I deem it proper to keep my remarks brief and general, leaving criticism and appraisal to the active participants.

Operationalism is an attitude that emphasizes the need of recourse, wherever feasible, to instrumental procedures when meanings are to be established. Bridgman disavows its status as a philosophy and wisely so, for as a general view it would be vulnerable on two counts. First, it cannot define the meaning of "instrumental procedure" in a manner that saves the view from being either trivial (which would be true if "instrumental" were construed to include symbolic, mental and paper-and-pencil operations) or too restrictive (if all operations are to be laboratory procedures). Second, it fails to impart meaning to substantive concepts—that is, concepts related to entities that are regarded as the carriers of operationally determinable qualities or quantities. To illustrate this latter point: it is possible to define, in terms of instrumental procedures, the charge, the mass, and the spin of an electron, but hardly the electron itself.

Yet every scientist feels the value of the operational approach. I shall try to indicate the reason for this by showing that operational definitions occupy a critical role in the methodology of science.

If, as is customary in much of traditional philosophy, we recognize within our cognitive experience two dominating poles, the *rational* (concepts, constructs, ideas, and so on) and the *immediate* (such as sensations, observations, and data), then there arises the problem of bridging the two. For it is clear that a concept is not identical with, or inductively derivable from, a set of percepts. The nonempirical requirements that render fertile and consistent the constructs of a given theory [in *The Nature of Physical Reality* (McGraw-Hill, 1950) I have called them "metaphysical requirements"] can be stated without reference to actual observations; hence, they do not validate or reify the constructs they confine but establish them merely as an internally consistent set, a formal theory. The empirical requirement, the possibility of circuits of factual verification that mediate between observational data and constructs, confers validity, and

such circuits are impossible without operational definitions—operational in terms of performed or imagined *laboratory procedures*.

This implies that operations are not the only means of defining scientific concepts; indeed, the analysis shows precisely why they alone are insufficient. A valid concept must belong to a satisfactory theory which obeys the metaphysical rules. This it cannot do unless it is susceptible to a formal definition that links it nonoperationally to other concepts or terms of the theory. But it must also be empirically verifiable, and this requires a linkage with observations via operational procedures.

Thus it is necessary, and a survey of scienti method shows this to be true, that every accep scientific measurable quantity have at least definitions, one formal and one instrumental. I an interesting task to show how some sciences to become exact because they ignore this dual ch acter of the definitory process. Omission of ope tional definitions leads to sterile speculation, metaphysics in the sense of the detractors of t discipline; disregard of formal (or "constitutive definitions leads to that blind empiricism wh misses the power and the beauty of modern ph ical science.

Sense and Nonsense in Operationism

GUSTAV BERGMANN

Dr. Bergmann, who received his Ph.D. degree in mathematics at the Univers of Vienna, is a member of the Vienna Circle, a group of philosophers that has come internationally famous. He came to this country in 1938 and, since 19 has been connected with the State University of Iowa, where he is now profes of philosophy and psychology. His primary interest is in the theory of knowled but he has also done much work in the philosophy of science, particularly in t logic of psychology. Dr. Bergmann has contributed chapters to several books a published numerous articles in philosophical, psychological, and mathemati journals. Some of these have been anthologized in H. Feigl and M. Brodbec Readings in the Philosophy of Science *(Appleton-Century-Crofts, New Yor 1953) and in M. Marx,* Psychological Theory *(Macmillan, New York, 1951 Eighteen of his epistemological essays have been published under the title Th* Metaphysics of Logical Positivism *(Longmans, Green, New York, 1954).*

A PHILOSOPHY or philosophic position is a system of mutually consistent and interdependent explications of and answers to philosophic questions with the implicit claim that all such questions—those that have been asked in the past as well as those that may be asked in the future—can be clarified in the style or manner of the system. The number of points actually dealt with must, therefore, be large enough to provide an adequate idea of the style or manner. By this token, operationism is not a philosophic position. Its concern is with one point and one point only. More conservatively still, operationism, in any sense both reasonable and reasonably specific, is merely a footnote, though an important one, to a point that has received much attention and has been accepted by many philosophers, at least since Locke first distinguished between simple and complex ideas. I shall first state the point, then the footnote.

Imagine one who knows the grammar of our language and understands its logical words bu understands only a part of its descriptive vocabu lary. Call this part his *basic descriptive vocabulary*. If this basis is properly chosen, such a one can, i principle, be taught to understand any descripti term by presenting him, within our language, wi its definition. This is the point. In stating it I ma aged to avoid, at least verbally, the controversia notion of meaning. I did this because I believe th some of the nonsense that is now being said abou meaning is not unconnected with some vagu notion of operationism. To this sort of thing I sha briefly attend in my last comment. But the notio of definition, which I could not avoid, is also con troversial, not perhaps like meaning in first phi losophy, yet precisely in the context in which used it. I add, then, that I use it at the momen in a sense broad enough to preclude controvers This broad sense includes definitions proper speaking, that is, definitions whose definienda a eliminable; it includes partial definitions, wha

…er it may mean for a definition to be partial, that is, so-called reduction pairs; it even includes what more commonly, and I think less confusingly, is called the interpretation of axiomatic calculi. In this broad sense of definition, the point is, to repeat, the definability of our descriptive vocabulary in terms of a part of it.

The footnote is a comment on the logic of concept formation in science. This limitation of range has two consequences. For one, the terms whose definability or definitions are asserted or examined are merely the concepts of the several sciences, particularly, as one says, very loosely but quite intelligibly, the more abstract ones. For another, the basic vocabulary may be assumed to contain only the simplest terms of what Carnap once called the thing language. Informally speaking, it does not exceed that of an unsophisticated laboratory attendant. Consider now, without prejudice, the case of an explicit definition, say, that of a very primitive notion of length. The definition being of the kind called in use, the definiendum is not the single term 'length' (1) but a whole sentence, say, in an instance, 'The length of this ledge is three feet.' The definiens proposed is a conditional, 'If *A* then *B*' where '*A*' is a statement of the measuring procedure, namely, the laying off in a certain manner of a foot rule, and '*B*' a statement of what one observes after one has performed this operation, namely, the coincidence of one corner of the ledge with one end of the foot rule at the third step. Generally one may say, again without prejudice as to detail, that the definiens states what one must do and what, having done it, one must see in order to assert the truth of the simplest kind of statement in which the new term occurs. This comment, with its emphasis on what we do and that we often must do something, manipulatively, if we want to find out whether a certain statement is true, is the important footnote operationism has contributed.

All this is completely noncontroversial. Equally noncontroversial is, to my mind, the proper answer to an objector who voices, as it were, certain apprehensions of common-sense realism. "What you say," the objection goes, "sounds suspiciously as if you thought you were somehow making the ledge's length when you measured it. Yet, the ledge has a length that is there and, in particular, the length of three feet whether or not you or anybody else measures it." Here is what I would answer. "The query for what there is, the so-called ontological question, belongs to first philosophy. It cannot even be discussed intelligently in this narrow context, and the logic of scientific concept formation, with or without the operationist footnote, has no bearing on it whatsoever. If it will help to convince you that this is so, I am prepared, as long as we talk in this very general fashion, to put the conditional in the subjunctive, 'If one *were* to do this and this, then one *would* observe that and that.' On the other hand, if you want to *know* how long the ledge is and have no means of inferring it from something else that you already know, then you will indeed have to measure it. This is perfectly commonsensical. So you need not fear that by admitting it you have fallen into a philosophic trap."

Fundamentally, this is all that needs to be said about operationism as such. If, however, one is to survey sense and nonsense around it, one should, perhaps, do three more things. First, one may take stock of the more technical disagreements, on which both sides make sense. Second, one may explore, as an analytic historian of recent ideas, the impact of the doctrine on the several sciences. Third, one should at least identify some of the philosophic nonsense that was either stimulated by or is in some fashion congenial to an exuberant notion of operationism. I shall briefly take up these three topics in this order.

I

The finer technical points all hinge on how broad a notion of definition the thesis requires. In taking stock of them, as I said I would, rather than discussing them once more in detail, my main purpose is to trace the disagreements in this limited area to the several positions taken on certain philosophic issues, issues that are much more pervasive and fundamental.

Some concepts are introduced not by definitions in the narrower sense, either complete or incomplete, but by the interpretation of axiomatic calculi. On this, as far as I know, everybody who is at all interested in these questions agrees. Everybody agrees, for instance, though perhaps not for the same reasons, that the terms of quantum mechanics must be so introduced. There is disagreement, and that not very serious, only on how much can be achieved without resorting to this procedure. The salient feature of the latter is the freedom it affords to introduce terms that have by themselves no empirical counterparts, that is, counterparts presumably introduced by chains of definitions in the narrower sense. Let me sacrifice pointless generality to the advantages of an illustration and consider the kinetic theory of gases of, say, 1890. The empirical terms are in this case those of phenomenological thermodynamics; the calculus is the so-called mechanical model. Such terms and phrases as 'molecule,' 'the position of a molecule,'

and 'the momentum of a molecule' have by themselves no corresponding empirical terms. This peculiarity is now sometimes being talked about in a manner I find confusing. Specifically, I would not say, as some who say certain things now would have had to say consistently had they lived in 1890, that it was then "operationally meaningless" to attribute position or momentum to an individual molecule. Saying any such thing implies a censure that is completely unjustified, irrespective of the eventual success or failure of the theory. Or, to use for once that dangerous word, what is here involved is merely a question of economy, not the much more radical one of meaning. The lack of economy one may charge, which is now sometimes spoken of as the excess meaning of a model, has, of course, its advantages as well as its price.

Concerning the question how soon, as it were, one is reduced to the interpretation of axiomatic calculi, I wish to comment on a recent statement by Hempel (2). Examining in detail how real numbers enter into scientific statements, he first reminds us, with his usual lucidity, of the gap that separates real numbers from the crude fractions we can manipulatively represent and then argues that the accurate bridging of this gap requires the interpretation of axiomatic calculi. I have, of course, no quarrel with the argument. I merely deplore the emphasis. I deplore it because I believe that if in any given case the use of real numbers were the only reason why definitions in the narrower sense break down, it would still be worth while to investigate how far we could get by definitions alone if we forewent the convenience of real numbers. In other words, one should try to bring out the differences between, say, length and a ψ-function, rather than put them in the same boat, for a reason that is fairly obvious. For the real numbers are merely a part of the logical apparatus; concept formation is a matter of the descriptive vocabulary. So it will be well to put to one side the specific problems of the former in order to get an unencumbered view of the latter. To say the same thing differently, one may start with the interpretation of an axiomatic calculus, so that real numbers can be used once and for all, and then ask how much can be done in this calculus by means of definitions alone. But then, all this goes to show that my demurrer is merely a matter of perspective. It is not in itself a disagreement on either fact or logic.

Those who disagree on the respective scope and merit of explicit definition and reduction pairs also share common ground. In fact, it is on this ground that the issue must eventually be joined. Both sides believe that for a technical discussion it is necessary to construct formalisms or ideal languages that are, in a familiar sense, abstracts or schematic reconstructions of our natural language. For the rest, there are two issues in this area, not one. An explicit definition is also complete in that, if it is once stated, nothing can be added to it; to add to it or to change it otherwise is to propose an alternative reconstruction. Reduction pairs may be supplemented by further reduction pairs. The one issue is thus completeness versus incompleteness in this sense. The other is eliminability versus noneliminability, for the definienda of explicit definitions are eliminable. Reduction pairs lack this feature and are, therefore, not literally definitions in the traditional logical sense. I turn first to the second of these issues.

The facts in the case, if I may so call them, are as noncontroversial as the so-called paradoxes of material implication from which they ultimately flow. In those formalisms that the two sides are willing to accept, it is impossible to reconstruct with idiomatic accuracy the subjunctive, 'If one *were* to do this, one *would* see that,' which I just proffered in my imaginary exchange to the suspicious realist. Technically this entails that if explicit definitions are used in the reconstruction, certain statements become in certain cases false when their idiomatic counterparts are true, and conversely; when reduction pairs are used, their truth values remain in these cases conveniently indeterminate. The advocates of explicit definition maintain that these disturbing cases are really quite trivial and that therefore, if their mechanism is once understood, one need not worry about them. The philosophic motives behind this stand, which as I said by far transcend the immediate issue, are two. For one, these students feel that a so-called definition which abandons eliminability is not a satisfactory analysis of the term it introduces. For another, they are convinced that the formal schema cannot and need not in all details and trait by trait agree with the idiom. For if it did, how could it serve its one and only purpose, to be a tool of philosophic analysis and not, phantastically, an artificial language to be actually spoken? So the advocates of explicit definition, of which I am one (3), avail themselves at this point of the leeway that conviction gives them.

Some see a virtue in the incompleteness of reduction pairs. They reason as follows. As, say, electrodynamics developed, more and more ways of measuring current were discovered. Each of these

may be thought to provide a new reduction pair. As more and more of these pairs accrue to what remains in a sense the same "definition," the concept defined becomes richer and richer in meaning. This, by the way, is a harmless meaning of 'meaning,' a less slippery synonym being 'significance.' Those who argue this way reject explicit definitions because of their completeness. Supposedly they are too static and cannot do justice to the growing edge of knowledge. The counterargument moves on two levels. First and in principle, the logical analysis of knowledge is not concerned with its growth. To explain this growth is the task of the behavior sciences. To confuse the latter with logical analysis is to adopt the instrumentalist or some other variant of the idealistic position. Second and in detail, the significance of a concept at any given stage of knowledge reflects itself, not in its definition, but in the laws in which it occurs.

Recently incomplete definitions have found a new advocate in Pap (*4*). According to Pap, the real trouble with complete ones is that they do not allow for the assignment of the proper probabilities, of the kind now called 'probability$_1$,' to scientific theories. That may well be so. I make no claim to expertness on the details of probability$_1$. Only, one who thinks, as I do (*5*), that probability$_1$ is a blind alley, will naturally not be impressed by the new twist. This, however, is a different story and, again, one far more important than what is directly at stake between the critics and the defenders of reduction pairs.

II

Operationism, both the idea and the word, originated within physics. Its manifesto, Bridgman's *The Logic of Modern Physics,* was written by a physicist. The immediate stimulus apparently was Einstein's celebrated analysis of nonlocal simultaneity; understandably enough, since the best way of characterizing his achievement in a very general manner is to say that, having recognized the need for an operational definition of the concept, he proposed one that proved spectacularly significant. Yet, with the Einsteinian revolution consummated, the physical sciences did not stand in great need of the operationist discipline. Accordingly the impact was limited. Occasionally one finds in physicists' writings statements of the kind I criticized when I used the kinetic theory of 1890 as an illustration. I do not think that this kind of mild nonsense or an occasional entirely nonspecific use of the word 'operationism' does much harm in the physical sciences. They are much too set for that.

The impact on psychology was tremendous. Again this is easily understood. Applied to psychological concepts, operationism becomes methodological behaviorism, that is, a behaviorism sobered and shorn of its metaphysics. Operationism can thus take credit for having facilitated the transition from Watsonianism to contemporary behavior theory. To be sure, there was also some nonsense, mostly misunderstandings owing to the philosophic naivety of some psychologists. By now these misunderstandings have happily disappeared; at least, they have been pointed out. The root of the trouble was that some psychologists in their enthusiasm mistook the operationist footnote for the whole philosophy of science, if not for the whole of philosophy. So they thought, first, that operationism also provided rules for assuring the significance of concepts properly defined. There are, of course, no such rules. Second, while operations in the relevant sense are manipulations and nothing else, they saw operations everywhere. At the one extreme, the scientist's perceptions were decked out to be a species of operations; at the other, his verbal and computational activities were as so-called symbolic operations herded into the same corral. This completely nonspecific use of 'operation' proved confusing. And there was still another confusion. To give an extreme illustration, some refused, presumably on operationist principles, to "generalize" from one instance of an experiment to the next if the apparatus had in the meantime been moved to another corner of the room. Yet, there is no *a priori* rule to distinguish relevant from irrelevant variables. Nor is there any such thing as an exhaustive description. Generally, the operationist fashion provided some specious arguments to those who disliked all sorts of theorizing or, even, conceptualizing. But perhaps it was wholesome that psychology went through this phase.

If physics did not particularly need the operationist discipline and if psychology has accepted it, the case of the group sciences such as economics and sociology is again different. Their outstanding feature is the occurrence of group concepts. The one kind, call it 'statistical,' is exemplified by, say 'average income' and 'export import ratio.' These can obviously be defined statistically in terms of psychological and environmental concepts. Such definability, whether statistical or otherwise, is at the moment not so obvious for the second kind. This kind, call it 'institutional,' is exemplified by, say, 'the Church' and 'the moral code of the Army.' Yet I believe, as probably most philosophers in the empiricist tradition do, that there are only two

alternatives. Either institutional concepts are so vague that one may as well give up hope of ever incorporating them into a worth-while science; or they, too, will eventually be defined, or, if you please, operationally defined in terms of psychological and environmental concepts. If I am not mistaken, the group sciences are only now, and not without some resistance, absorbing this idea (*6*). So the discipline of operationist thinking may still do them some good.

III

The philosophic nonsense somehow connected with some vague notion of operationism is of two main varieties: the one, an instrumentalist misinterpretation; the other, the so-called operational theory of meaning. The latter, too, has instrumentalist affinities.

Instrumentalism stems from an exaggerated and subtly twisted emphasis on doing or manipulating. Practically, experimentation is indispensable in science, just as we must act if we want to survive. In principle, however, and I put it strongly in order to make the point, if only we lived long enough and were patient enough, we could choose to remain spectators and wait until those situations that we in fact so ingeniously contrive occur, as one says, by chance. Logically, what matters is that they happen, not that we make them happen. This alone shows that the operationist footnote has no tendency whatsoever to give aid and comfort to the strange pseudoscientific subjectivism which wants us to believe that we somehow by our thoughts and actions make or determine what is in itself an indeterminate situation. I, for one, find myself in a world that is quite determinate, not at all of my making and, alas, quite often not to my liking.

Bridgman's well-known formula, "In general, we mean by any concept nothing more than a set of operations; the concept is synonymous with the corresponding set of operations," is perhaps merely a scientist's version of a so-called meaning criterion. So interpreted, it excludes from science terms not defined from a thing-language basic vocabulary. If, as the words would indicate, it excludes the terms of interpreted axiomatic calculi, then it is unduly restrictive. If it is to include them, then the notion of an operation must, as I just observed, be stretched beyond all reason. But Bridgman's formula could also be a scientist's version of Wittgenstein's dictum that the meaning of a proposition is the method of its verification. So interpreted, it becomes a statement of the verification theory of meaning. A so-called theory of meaning, by the way, is not the same thing as a criterion of meaning. A criterion says which terms have meaning; a theory says what meaning is. Perhaps a verification theory was what Bridgman had in mind. I do not know. His formula lends itself to still another interpretation. Taken literally, it identifies the meaning of a concept not with its referent, but with the operations one must perform if one wants to test the presence of this referent. So interpreted, it is an instrumentalist variant of the standard reference theory. Unfortunately this third reading has been the most influential. It is without doubt one of the major sources of all the nonsensical talk that passes for an operational theory of meaning. What seemed attractive in it was probably the substitution of something comfortably concrete, operations, for something suspiciously abstract, meaning.

Let me say one word in conclusion. Even if one discounts some meanings that are not difficult to analyze, 'meaning' is still not a univocal term. Thus no univocal or one-track theory will do. Philosophers who adopt one must blunder somewhere sooner or later. But this is not to say that all univocal theories are, like the operational one, in themselves clouds of confusion. They may, and I think some of them do, explicate important meanings of meaning. This, however, is another story (*7*).

References and Notes

1. In this article, single quotation marks are used to enclose a word or expression that is mentioned, not used. Double quotation marks indicate either emphasis or direct quotation.
2. C. G. Hempel, *Foundations of Concept Formation in Empirical Science* (Univ. of Chicago Press, Chicago, 1952).
3. G. Bergmann, "Comments on Professor Hempel's 'The concept of cognitive significance,'" *Proc. Am. Acad. Arts Sci.* **80**, 78 (1951); "Comments on Storer's definition of 'soluble,'" *Analysis* **12**, 44 (1951).
4. A. Pap, "Reduction sentences and open concepts," *Methodos* **5**, 3 (1953).
5. G. Bergmann, "Some comments on Carnap's logic of induction," *Phil. Sci.* **13**, 71 (1946).
6. A contribution of this kind is made in H. D. Lasswell and A. Kaplan, *Power and Society* (Yale Univ. Press, New Haven, Conn., 1950). See also my review of this book in *Ethics* **52**, 64 (1951). For a systematic discussion of these issues, including so-called reduction, see M. Brodbeck, "On the philosophy of the social sciences," *Phil. Sci.* **21**, 140 (1954).
7. "Logical positivism, language, and the reconstruction of metaphysics," *Riv. Critica di Storia della Filosofia* 8, 453 (1953) [reprinted in my book, *The Metaphysics of Logical Positivism* (Longmans, Green, New York, 1954)].

A Logical Appraisal of Operationism

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OPERATIONISM, in its fundamental tenets, is closely akin to logical empiricism. Both schools of thought have put much emphasis on definite experiential meaning or import as a necessary condition of objectively significant discourse, and both have made strong efforts to establish explicit criterions of experiential significance. But logical empiricism has treated experiential import as a characteristic of statements—namely, as their susceptibility to test by experiment or observation—whereas operationism has tended to construe experiential meaning as a characteristic of concepts or of the terms representing them—namely, as their susceptibility to operational definition.

Basic ideas of operational analysis. An operational definition of a term is conceived as a rule to the effect that the term is to apply to a particular case if the performance of specified operations in that case yields a certain characteristic result. For example, the term 'harder than' might be operationally defined by the rule that a piece of mineral, x, is to be called harder than another piece of mineral y, if the operation of drawing a sharp point of x across the surface of y results in a scratch mark on the latter. Similarly, the different numerical values of a quantity such as length are thought of as operationally definable by reference to the outcomes of specified measuring operations. To safeguard the objectivity of science, all operations invoked in this kind of definition are required to be intersubjective in the sense that different observers must be able to perform "the same operation" with reasonable agreement in their results (*1*).

P. W. Bridgman, the originator of operational analysis, distinguishes several kinds of operation that may be invoked in specifying the meanings of scientific terms (*2*). The principal ones are (i) what he calls *instrumental operations*—these consist in the use of various devices of observation and measurement—and (ii) paper-and-pencil operations, verbal operations, mental experiments, and the like—this group is meant to include, among other things, the techniques of mathematical and logical inference as well as the use of experiments in imagination. For brevity, but also by way of suggesting a fundamental similarity among the procedures of the second kind, I shall refer to them as *symbolic operations*.

The concepts of operation and of operational definition serve to state the basic principles of operational analysis, of which the following are of special importance.

1) "Meanings are operational." To understand the meaning of a term, we must know the operational criterions of its application (*3*), and every meaningful scientific term must therefore permit of an operational definition. Such definition may refer to certain symbolic operations and it always must ultimately make reference to some instrumental operation (*4*).

2) To avoid ambiguity, every scientific term should be defined by means of one unique operational criterion. Even when two different operational procedures (for instance, the optical and the tactual ways of measuring length) have been found to yield the same results, they still must be considered as defining different concepts (for example, optical and tactual length), and these should be distinguished terminologically because the presumed coincidence of the results is inferred from experimental evidence, and it is "not safe" to forget that the presumption may be shown to be spurious by new, and perhaps more precise, experimental data (*5*).

3) The insistence that scientific terms should have unambiguously specifiable operational meanings serves to insure the possibility of an objective test for the hypotheses formulated by means of those terms (*6*). Hypotheses incapable of operational test or, rather, questions involving untestable formulations are rejected as meaningless: "If a specific question has meaning, it must be possible to find operations by which an answer may be given to it. It will be found in many cases that the operations cannot exist, and the question therefore has no meaning." (*7*)

The emphasis on "operational meaning" in

scientifically significant discourse has unquestionably afforded a salutary critique of certain types of procedure in philosophy and in empirical science and has provided a strong stimulus for methodological thinking. Yet, the central ideas of operational analysis as stated by their proponents are so vague that they constitute not a theory concerning the nature of scientific concepts but rather a program for the development of such a theory. They share this characteristic with the insistence of logical empiricism that all significant scientific statements must have experiential import, that the latter consists in testability by suitable data of direct observation, and that sentences which are entirely incapable of any test must be ruled out as meaningless "pseudo hypotheses." These ideas, too, constitute not so much a thesis or a theory as a program for a theory that needs to be formulated and amplified in precise terms.

An attempt to develop an operationist theory of scientific concepts will have to deal with at least two major issues: the problem of giving a more precise explication of the concept of operational definition; and the question whether operational definition in the explicated sense is indeed necessary for, and adequate to, the introduction of all nonobservational terms in empirical science.

I wish to present here in brief outline some considerations that bear on these problems. The discussion will be limited to the descriptive, or extralogical, vocabulary of empirical science and will not deal, therefore, with Bridgman's ideas on the status of logic and mathematics.

A broadened conception of operational definition and of the program of operational analysis. The terms 'operational meaning' and 'operational definition,' as well as many of the pronouncements made in operationist writings, convey the suggestion that the criterions of application for any scientific term must ultimately refer to the outcome of some specified type of manipulation of the subject matter under investigation. Such emphasis would evidently be overly restrictive. An operational definition gives experiential meaning to the term it introduces because it enables us to decide on the applicability of that term to a given case by observing the response the case shows under specifiable test conditions. Whether these conditions can be brought about at will by "instrumental operations" or whether we have to wait for their occurrence is of great interest for the practice of scientific research, but it is inessential in securing experiental import for the defined term; what matters for this latter purpose is simply that the relevant test conditions and the requisite response be of such kind that different investigators can ascertain, by direct observation and with reasonably good agreement, whether, in a given case, the test conditions are realized and whether the characteristic response does occur.

Thus, an operational definition of the simplest kind—one that, roughly speaking, refers to instrumental operations only—will have to be construed more broadly as introducing a term by the stipulation that it is to apply to all and only those cases which, under specified observable conditions S, show a characteristic observable response R.

However, an operational definition cannot be conceived as specifying that the term in question is to apply to a given case only if S and R actually occur in that case. Physical bodies, for example, are asserted to have masses, temperatures, charges, and so on, even at times when these magnitudes are not being measured. Hence, an operational definition of a concept—such as a property or a relationship, for example—will have to be understood as ascribing the concept to all those cases that *would* exhibit the characteristic response if the test conditions *should* be realized. A concept thus characterized is clearly not "synonymous with the corresponding set of operations" (8). It constitutes not a manifest but a potential character, namely, a disposition to exhibit a certain characteristic response under specified test conditions.

But to attribute a disposition of this kind to a case in which the specified test condition is not realized (for example, to attribute solubility-in-water to a lump of sugar that is not actually put into water) is to make a generalization, and this involves an inductive risk. Thus, the application of an operationally defined term to an instance of the kind here considered would have to be adjudged "not safe" in precisely the same sense in which Bridgman insists it is "not safe" to assume that two procedures of measurement that have yielded the same results in the past will continue to do so in the future. It is now clear that if we were to reject any procedure that involves an inductive risk, we would be prevented not only from using more than one operational criterion in introducing a given term but also from ever applying a disposition term to any case in which the characteristic manifest conditions of application are not realized; thus, the use of dispositional concepts would, in effect, be prohibited.

A few remarks might be added here concerning the noninstrumental operations countenanced for the introduction especially of theoretical terms. In operationist writings, those symbolic procedures

have been characterized so vaguely as to permit the introduction, by a suitable choice of "verbal" or "mental" operations, of virtually all those ideas that operational analysis was to prohibit as devoid of meaning. To meet this difficulty, Bridgman has suggested a distinction between "good" and "bad" operations (*9*); but he has not provided a clear criterion for this distinction. Consequently, this idea fails to plug the hole in the operationist dike.

If the principles of operationism are to admit the theoretical constructs of science but to rule out certain other kinds of terms as lacking experiential, or operational, meaning, then the vague requirement of definability by reference to instrumental and "good" symbolic operations must be replaced by a precise characterization of the kinds of sentences that may be used to introduce, or specify the meanings of, "meaningful" nonobservational terms on the basis of the observational vocabulary of science. Such a characterization would eliminate the psychologistic notion of mental operations in favor of a specification of the logico-mathematical concepts and procedures to be permitted in the context of operational definition.

The reference just made to the observational vocabulary of science is essential to the idea of operational definition; for it is in terms of this vocabulary that the test conditions and the characteristic response specified in an operational definition are described and by means of which, therefore, the meanings of operationally defined terms are ultimately characterized. Hence, the intent of the original operationist insistence on intersubjective repeatability of the defining operations will be respected if we require that the terms included in the observational vocabulary must refer to attributes (properties and relationships) that are directly and publicly observable—that is, whose presence or absence can be ascertained, under suitable conditions, by direct observation, and with good agreement among different observers (*10*).

In sum, then, a precise statement and elaboration of the basic tenets of operationism require an explication of the logical relationships between theoretical and observational terms, just as a precise statement and elaboration of the basic tenets of empiricism require an explication of the logical relationships connecting theoretical sentences with observation sentences describing potential data of direct observation.

Specification of meaning by explicit definition and by reduction. Initially, it may appear plausible to assume that all theoretical terms used in science can be fully defined by means of the observational vocabulary. There are various reasons, however, to doubt this assumption.

First of all, there exists a difficulty concerning the definition of the scientific terms that refer to dispositions—and, as is noted in a foregoing paragraph, all the terms introduced by operational definition have to be viewed as dispositional in character. Recent logical studies strongly suggest that dispositions can be defined by reference to manifest characteristics, such as those presented by the observational vocabulary, only with help of some "nomological modality" such as the concept of nomological truth, that is, truth by virtue of general laws of nature (*11*). But a concept of this kind is presumably inadmissible under operationist standards, since it is neither a directly observable characteristic nor definable in terms of such characteristics.

Another difficulty arises when we attempt to give full definitions, in terms of observables, for quantitative terms such as 'length in centimeters,' 'duration in seconds,' 'temperature in degrees Celsius.' Within scientific theory, each of these is allowed to assume any real-number value within a certain interval; and the question therefore arises whether each of the infinitely many permissible values, say of length, is capable of an operational specification of meaning. It can be shown that it is impossible to characterize every one of the permissible numerical values by some truth-functional combination of observable characteristics, since the existence of a threshold of discrimination in all areas of observation allows for only a finite number of nonequivalent combinations of this kind (*12*).

Difficulties such as these suggest the question whether it is not possible to conceive of methods more general and more flexible than definition for the introduction of scientific terms on the basis of the observational vocabulary. One such method has been developed in considerable detail by Carnap. It makes use of so-called reduction sentences, which constitute a considerably generalized version of definition sentences and are especially well suited for a precise reformulation of the intent of operational definitions. As we noted earlier, an operational definition of the simplest kind stipulates that the concept it introduces, say *C*, is to apply to those and only those cases which, under specified test conditions *S*, show a certain characteristic response *R*. In Carnap's treatment, this stipulation is replaced by the sentence

$$Sx \rightarrow (Cx \equiv Rx) \qquad (1)$$

or, in words: If a case *x* satisfies the test condition

S, then x is an instance of C if and only if x shows the response R. Formula 1, called a bilateral reduction sentence, is not a full definition (which would have to be of the form '$Cx \equiv \ldots$,' with 'Cx' constituting the definiendum); it specifies the meaning of 'Cx,' not for all cases, but only for those that satisfy the condition S. In this sense, it constitutes only a partial, or conditional, definition for C (*13*). If 'S' and 'R' belong to the observational vocabulary of science, formula 1 schematizes the simplest type of operational definition, which invokes (almost) exclusively instrumental operations or, better, experiential findings. Operational definitions that also utilize symbolic operations would be represented by chains of reduction sentences containing logical or mathematical symbols. Some such symbols occur even in formula 1, however; and clearly, there can be no operational definition that makes use of no logical concepts at all.

Interpretative systems. Once the idea of a partial specification of meaning is granted, it appears unnecessarily restrictive, however, to limit the sentences effecting such partial interpretation to reduction sentences in Carnap's sense. A partial specification of the meanings of a set of nonobservational terms might be expressed, more generally, by one or more sentences that connect those terms with the observational vocabulary but do not have the form of reduction sentences. And it seems well to countenance, for the same purpose, even stipulations expressed by sentences containing only nonobservational terms; for example, the stipulation that two theoretical terms are to be mutually exclusive may be regarded as a limitation and, in this sense, a partial specification of their meanings.

Generally, then, a set of one or more theoretical terms, $t_1, t_2, \ldots, t_n$, might be introduced by any set M of sentences such that (i) M contains no extralogical terms other than $t_1, t_2, \ldots, t_n$, and observation terms, (ii) M is logically consistent, and (iii) M is not equivalent to a truth of formal logic. The last two of these conditions serve merely to exclude trivial extreme cases. A set M of this kind will be referred to briefly as an *interpretative system*, its elements as *interpretative sentences*.

Explicit definitions and reduction sentences are special types of interpretative sentences, and so are the meaning postulates recently suggested by Kemeny and Carnap (*14*).

The interpretative sentences used in a given theory may be viewed simply as postulates of that theory (*15*), with all the observation terms, as well as the terms introduced by the interpretative system, being treated as primitives. Thus construed, the specification of the meanings of nonobservational terms in science resembles what has sometimes been called the implicit definition of the primitives of an axiomatized theory by its postulates. In this latter procedure, the primitives are all uninterpreted, and the postulates then impose restrictions on any interpretation of the primitives, that is to turn the postulates into true sentences. Such restrictions may be viewed as partial specifications of meaning. The use of interpretative systems as here envisaged has this distinctive peculiarity, however: the primitives include a set of terms—the observation terms—which are antecedently understood and thus not in need of an interpretation, and by reference to which the postulates effect a partial specification of meaning for the remaining, nonobservational, primitives. This partial specification again consists in limiting those interpretations of the nonobservational terms that will render the postulates true.

Implications for the idea of experiential meaning and for the distinction of analytic and synthetic sentences in science. If the introduction of nonobservational terms is conceived in this broader fashion, which appears to accord with the needs of a formal reconstruction of the language of empirical science, then it becomes pointless to ask for the operational definition or the experiential import of any one theoretical term. Explicit definition by means of observables is no longer generally available, and experiential—or operational—meaning can be attributed only to the set of all the nonobservational terms functioning in a given theory.

Furthermore, there remains no satisfactory general way of dividing all conceivable systems of theoretical terms into two classes: those that are scientifically significant and those that are not; those that have experiential import and those that lack it. Rather, experiential, or operational, significance appears as capable of gradations. To begin with one extreme possibility: the interpretative system M introducing the given terms may simply be a set of sentences in the form of explicit definitions that provide an observational equivalent for each of those terms. In this case, the terms introduced by M have maximal experiential significance, as it were. In another case, M might consist of reduction sentences for the theoretical terms; these will enable us to formulate, in terms of observables, a necessary and a (different) sufficient condition of application for each of the introduced terms. Again M might contain sentences in the form of definitions or reduction sentences for only some of the nonobservational terms it introduces. And finally, none of the sentences in M

might have the form of a definition or of a reduction sentence: and yet, a theory whose terms are introduced by an interpretative system of this kind may well permit of test by observational findings, and in this sense, the system of its nonobservational terms may possess experiential import (*16*). Thus, experiential significance presents itself as capable of degrees, and any attempt to set up a dichotomy allowing only experientially meaningful and experientially meaningless concept systems appears as too crude to be adequate for a logical analysis of scientific concepts and theories.

The use of interpretative systems is a more inclusive method of introducing theoretical terms than the method of meaning postulates developed by Carnap and Kemeny. For although meaning postulates are conceived as analytic and, hence, as implying only analytic consequences, an interpretative system may imply certain sentences that contain observation terms but no theoretical terms and are neither formal truths of logic nor analytic in the customary sense. Consider, for example, the following two interpretative sentences, which form what Carnap calls a reduction pair, and which interpret 'C' by means of observation predicates, 'R_1,' 'S_1,' 'R_2,' 'S_2':

$$S_1x \rightarrow (R_1x \rightarrow Cx) \qquad (2.1)$$
$$S_2x \rightarrow (R_2x \rightarrow -Cx). \qquad (2.2)$$

Since in no case the sufficient conditions for C and for $-C$ (non-C) can be satisfied jointly, the two sentences imply the consequence (*17*) that, for every case x,

$$-(S_1x \,.\, R_1x \,.\, S_2x \,.\, R_2x), \qquad (3)$$

that is, no case x exhibits the attributes S_1, R_1, S_2, R_2 jointly. Now, an assertion of this kind is not a truth of formal logic, nor can it generally be viewed as true solely by virtue of the meanings of its constituent terms. Carnap therefore treats this consequence of formulas 2.1 and 2.2 as empirical and as expressing the factual content of the reduction pair from which it was derived. Occurrences of this kind are by no means limited to reduction sentences, and we see that in the use of interpretative systems, specification of meaning and statement of empirical fact—two functions of language often considered as completely distinct—become so intimately bound up with each other as to raise serious doubt about the advisability or even the possibility of preserving that distinction in a logical reconstruction of science. This consideration suggests that we dispense with the distinction, so far maintained for expository purposes, between the interpretative sentences, included in M, and the balance of the sentences constituting a scientific theory: we may simply conceive of the two sets of sentences as constituting one "interpreted theory."

The results obtained in this brief analysis of the operationist view of significant scientific concepts are closely analogous to those obtainable by a similar study of the logical empiricist view of significant scientific statements, or hypotheses (*18*). In the latter case, the original requirement of full verifiability or full falsifiability by experiental data has to give way to the more liberal demand for confirmability—that is, partial verifiability. This demand can be shown to be properly applicable to entire theoretical systems rather than to individual hypotheses—a point emphasized, in effect, already by Pierre Duhem. Experiential significance is then seen to be a matter of degree, so that the originally intended sharp distinction between cognitively meaningful and cognitively meaningless hypotheses (or systems of such) has to be abandoned; and it even appears doubtful whether the distinction between analytic and synthetic sentences can be effectively maintained in a formal model of the language of empirical science.

References and Notes

1. P. W. Bridgman, "Some general principles of operational analysis" and "Rejoinders and second thoughts" *Psychol. Rev.* **52**, 246 (1945); "The nature of some of our physical concepts" *Brit. J. Phil. Sci.* **1**, 258 (1951).
2. ———, "Operational analysis" *Phil. Sci.* **5**, 123 (1938); *Brit. J. Phil. Sci.* **1**, 258 (1951).
3. ———, *Phil. Sci.* **5**, 116 (1938).
4. ———, *Brit. J. Phil. Sci.* **1**, 260 (1951).
5. ———, *The Logic of Modern Physics* (Macmillan, New York, 1927), pp. 6, 23–24; *Phil. Sci.* **5**, 121 (1938); *Psychol. Rev.* **52**, 247 (1945); "The operational aspect of meaning," *Synthèse* **8**, 255 (1950–51).
6. ———, *Psychol. Rev.* **52**, 246 (1945).
7. ———, *The Logic of Modern Physics*, p. 28.
8. ———, *ibid.*, p. 5; qualified by Bridgman's reply [*Phil. Sci.* **5**, 117 (1938)] to R. B. Lindsay, "A critique of operationalism in physics," *Phil. Sci.* **4**, (1937), a qualification that was essentially on the ground, quite different from that given in the present paper, that operational meaning is only a necessary, but presumably not a sufficient, characteristic of scientific concepts.
9. ———, *Phil. Sci.* **5**, 126 (1938); "Some implications of recent points of view in physics," *Rev. intern. phil.* **3**, 484 (1949). The intended distinction between good and bad operations is further obscured by the fact that in Bridgman's discussion the meaning of "good operation" shifts from what might be described as "operation whose use in operational definition insures experiential meaning and testability" to "scientific procedure—in some very broad sense—which leads us to correct predictions."
10. The condition thus imposed upon the observational vocabulary of science is of a pragmatic character: it demands that each term included in that vocabulary be of such a kind that under suitable conditions,

different observers can, by means of direct observation, arrive at a high degree of agreement on whether the term applies to a given situation. The expression 'coincides with' as applicable to instrument needles and marks on scales of instruments is an example of a term meeting this condition. That human beings are capable of developing observational vocabularies that satisfy the given requirement is a fortunate circumstance: without it, science as an intersubjective enterprise would be impossible.

11. To illustrate briefly, it seems reasonable, *prima facie*, to define 'x is soluble in water' by 'if x is put in water then x dissolves.' But if the phrase 'if . . . then . . .' is here construed as the truth-functional, or "material," conditional, then the objects qualified as soluble by the definition include, among others, all those things that are never put in water—no matter whether or not they are actually soluble in water. This consequence—one aspect of the "paradoxes of material implication"—can be avoided only if the aforementioned definiens is construed in a more restrictive fashion. The idea suggests itself to construe 'x is soluble in water' as short for 'by virtue of some general laws of nature, x dissolves if x is put in water,' or briefly, 'it is nomologically true that if x is put in water then x dissolves.' The phrase 'if . . . then . . .' may now be understood in the truth-functional sense again. However, the acceptability of this analysis depends, of course, upon whether nomological truth can be considered as a sufficiently clear concept. For a fuller discussion of this problem complex, see especially R. Carnap, "Testability and meaning," *Phil. Sci.* **3** (1936) and **4** (1937) and N. Goodman, "The problem of counterfactual conditionals," *J. Phil.* **44** (1947).

12. In other words, it is not possible to provide, for every theoretically permissible value r of the length $l(x)$ of a rod x, a definition of the form

$$[l(x) = r] =_{df} C(P_1x, P_2x, \ldots, P_nx),$$

where $P_1, P_2, \ldots, P_n$ are observable characteristics, and the definiens is an expression formed from 'P_1x', 'P_2x', . . . , 'P_nx' with help of the connective words 'and,' 'or,' and 'not' alone.

It is worth noting, however, that if the logical constants allowed in the definiens include, in addition to truth-functional connectives, also quantifiers and the identity sign, then a finite observational vocabulary may permit the explicit definition of a denumerable infinity of further terms. For instance, if 'x spatially contains y' and 'y is an apple' are included in the observational vocabulary, then it is possible to define the expressions 'x contains 0 apples,' 'x contains exactly 1 apple,' 'x contains exactly 2 apples,' and so forth, in a manner familiar from the Frege-Russell construction of arithmetic out of logic. Yet even if definitions of this type are countenanced—and no doubt they are in accord with the intent of operationist analysis—there remain serious obstacles for an operationist account of the totality of real numbers which are permitted as theoretical values of length, mass, and so forth. On this point, see C. G. Hempel, *Fundamentals of Concept Formation in Empirical Science* (Univ. of Chicago Press, Chicago, 1952), sec. 7. Gustav Bergmann, in his contribution to the present symposium, deplores this argument—although he agrees with its point—on the ground that it focuses attention on a characteristic shared by all quantitative concepts instead of bringing out the differences between, say, length and the psi-function. He thinks this regrettable because, after all, as he puts it, "the real numbers are merely a part of the logical apparatus; concept formation is a matter of the descriptive vocabulary." I cannot accept the suggestion conveyed by this statement. To be sure, the theory of real numbers can be developed as a branch (or as an extension) of logic; however, my argument concerns not the definability of real numbers in logical terms, but the possibility of formulating an observational equivalent for each of the infinitely many permissible real-number values of length, temperature, and so forth. And this is clearly a question concerning the descriptive vocabulary rather than merely the logical apparatus of empirical science. I quite agree with Bergmann, however, that it would be of considerable interest to explicate whatever logical differences may obtain between quantitative concepts which, intuitively speaking, exhibit different degrees of theoretical abstractness, such as length on the one hand and the psi-function on the other.

13. The use of reduction sentences circumvents one of the difficulties encountered in the attempt to give explicit and, thus, complete definitions of disposition terms: the conditional and biconditional signs occurring in formula 1 may be construed truth-functionally without giving rise to undesirable consequences of the kind characterized in reference *11*. For details, see R. Carnap, "Testability and meaning," *Phil. Sci.* (1936–37), pt. II; also C. G. Hempel, *Fundamentals of Concept Formation in Empirical Science*, secs. 6 and 8. Incidentally, the use of nomological concepts is not entirely avoided in Carnap's procedure; the reduction sentences that are permitted for the introduction of new terms are required to satisfy certain conditions of logical or of nomological validity. See R. Carnap, *Phil. Sci.* **3** and **4** (1936–37), pp. 442–443.

14. J. G. Kemeny, "Extension of the methods of inductive logic," *Philosophical Studies* **3** (1952); R. Carnap, "Meaning postulates," *ibid.* **3** (1952).

15. For the case of Carnap's reduction sentences, the postulational interpretation was suggested to me by N. Goodman and by A. Church.

16. This is illustrated by the following simple model case: The theory T consists of the sentence '$(x)((C_1x \,.\, C_2x) \rightarrow C_3x)$' and its logical consequences; the three "theoretical" terms occurring in it are introduced by the interpretative set M consisting of the sentences '$O_1x \rightarrow (C_1x \,.\, C_2x)$' and '$(C_1x \,.\, C_3x) \rightarrow (O_2x \vee O_3x)$,' where '$O_1$,' '$O_2$,' '$O_3$' belong to the observational vocabulary. As is readily seen, T permits, by virtue of M, the "prediction" that if an object has the observable property O_1 but lacks the observable property O_2, then it will have the observable property O_3. Thus T is susceptible to experiential test, although M provides for none of its constituent terms both a necessary and a sufficient observational, or operational, criterion of application.

17. Carnap calls it the representative sentence of the pair of formulas 2.1 and 2.2. See R. Carnap, *Phil. Sci.* **3** and **4** (1936–37), p. 444 and p. 451. Generally, when a term is introduced by several reduction sentences representing different operational criterions of application, then the agreement among the results of the corresponding procedures, which must be presupposed if the reduction sentences are all to be compatible with one another, is expressed by the representative sentence associated with the given set of reduction sentences. The representative sentence reflects, therefore, the inductive risk which, as Bridgman has stressed, is incurred by using more than one operational criterion for a given term.

18. C. G. Hempel, "Problems and changes in the empiricist criterion of meaning," *Rev. intern. phil.* **4** (1951), and "The concept of cognitive significance: a reconsideration," *Proc. Am. Acad. Arts Sci.* **80** (1951); W. V. Quine, "Two dogmas of empiricism," *Phil. Rev.* **40**, (1951).

Operationalism in Physics Reassessed

R. B. LINDSAY

Dr. Lindsay, who has been Hazard professor of physics at Brown University since 1936, was recently appointed dean of the Graduate School. He is also director of the Ultrasonics Laboratory. He served as chairman of the physics department from 1934 to 1954. Dr. Lindsay received his training at Brown University and Massachusetts Institute of Technology, taking his Ph.D. in theoretical physics. He was a fellow of the American-Scandinavian Foundation in Copenhagen in 1922–23 and then taught physics at Yale University until 1930.

IT is scarcely necessary to emphasize the important role that the operational idea, as suggested and developed by P. W. Bridgman, has played in *physical* methodology during the past quarter-century. A token of its lively character is the fact that it can provide the background of a general symposium for the reassessment of the place of operationalism in philosophy as well as in physics. Many physical methodological points of view—for instance, Ostwald's energetics—arise, produce interest and excitement, have their day, and then are duly embalmed and laid away in the limbo of forgotten things. But Bridgman's ideas still challenge the attention of both scientists and scholars in many fields.

Before embarking on another critique, it may be worth while to indulge in a brief review of the historical situation; even if incomplete, this will in any event set the stage for the questions that I personally desire to raise.

Bridgman's stress on the value of the operational point of view was first brought widely to public notice in his book *The Logic of Modern Physics* (1927). His views were further developed in "A physicist's second reaction to Mengenlehre" [*Scripta Math.* **2,** 101, 224, (1934)] and in the later book *The Nature of Physical Theory* (1936). He never claimed that the idea was original with him and, indeed, in his first introduction of the notion stressed the essentially operational character of Einstein's treatment of the concepts of space and time in the relativity of inertial systems. Actually vestiges of the point of view, naturally not usually presented in the language Bridgman prefers to employ, may be found scattered throughout earlier literature on physical theorizing, as for instance in Galileo's *Two New Sciences*—recall his attempt to describe to his skeptical contemporaries the meaning and value of the concept of variable instantaneous velocity—or, to mention only two other examples, in the writings of Helmholtz on non-Euclidean geometry and those of W. K. Clifford on the concepts of space and time.

In essence what Bridgman was driving at can best be described in his own words, taken from a relatively recent publication (*1*):

> The fundamental idea back of an operational analysis is . . . that we do not know the meaning of a concept unless we can specify the operations which were used by us or our neighbor in applying the concept in any concrete situation.

It should at once be remarked that the author of this statement apparently at no time ever contemplated the foundation of a *new* philosophy of physics to be called *operationalism.* Actually he has shied away from this term and has preferred to speak and write merely of "operational analysis" or the operational point of view. When, however, one stresses with such emphasis a methodological technique of this kind, one must expect that it will gain attention, and Bridgman's views certainly have achieved this! It was probably inevitable that they would also suffer misinterpretation. In his anxiety to stress the operational technique as the only sure way of avoiding inconsistencies and contradictions in the use of physical concepts, it was perhaps natural to overstress the significance of actual physical operations in the laboratory. Many were misled into thinking that these were the only operations that Bridgman wished to tolerate and some critiques followed. At least one of these played devil's advocate in such thoroughgoing fashion that Bridgman wrote a reply ["Operational analysis," *Philosophy of Science* **5,** 114 (1938)] in which he cleared up doubtful points by explicitly including "paper-and-pencil" operations to cover many of the concepts constructed in the building of physical theories. He also conceded that the theory builder should be allowed all the latitude he wishes in these "intermediate constructions," provided only that "the ultimate outcome of the theory be expressible in terms of operations applicable in the concrete situation." I assume we may take this to mean that physical theorizing (if pursued with logical consistency) is free of all restrictions save only that

ultimately the results must be identifiable with laboratory operations and testable through their agreement or disagreement with the latter.

No theoretical physicist could, I believe, disagree with the foregoing interpretation of physical operationalism. One would have therefore supposed that the whole question might be considered closed, so far as physics is concerned. This was my view at that time (1938). It was therefore with great interest and not a little surprise that I studied the more recently published collection of papers *The Nature of Some of Our Physical Concepts* (Philosophical Library, New York, 1952), which were originally presented by Bridgman as lectures at University College in the University of London in 1950. Here the operational idea is applied in fascinating fashion to such concepts as field, action at a distance, heat, and entropy. Once more the distinction between "paper-and-pencil" and "instrumental" operations is clearly brought out, and the allowability of both kinds is admitted. But, as I read the discussion, there emerged a question that somehow the papers did not settle in my mind: How much latitude will Bridgman allow to the "free construction" of concepts? Although it is always a bit dangerous to quote out of context, the passage that particularly concerned me is this:

> It will be seen that a very great latitude is allowed to the verbal and the paper-and-pencil operation. I think, however, that physicists are agreed in imposing one restriction on the freedom of such operations, namely, that such operations must be capable of eventually, although perhaps indirectly, making connection with instrumental operations.

I do not know how to interpret this in any other way than that the author believes that ultimately the concepts and postulates of every successful physical theory shall be instrumentally verifiable. Admittedly the phrase "although perhaps indirectly" needs clarification. This then sets up the question I wish to ask Bridgman. Here lies actually the principal purpose of the present paper.

At this point perhaps it may be well to review briefly my own conception of physical explanation, which, so far as I know, is that of most theoretical physicists, although each man's phrasing is his own and there are always differences in emphasis at various points. Physical explanation ultimately is essentially deductive in character, consisting of theories whose aim is to predict the physical laws that describe physical experience. A physical theory starts with primitive, undefined concepts, such as the notions of space and time. It proceeds to the construction of more precisely defined constructs, for instance, mass and force in mechanics, into whose definition there enter both epistemic (operational) and constitutive (theoretical) characteristics. These aspects of the definition of physical constructs have been rather fully set forth by Henry Margenau (2) and will not be further elaborated at this point. The next step is the postulation of relationships connecting the constructs (for example, the principles of mechanics, like $F = ma$). Next in order are logical deductions by appropriate mathematical manipulation of relationships among quantities all of which have sufficient epistemic significance to be measurable in the laboratory. If these relationships are sufficiently general in character, they are called *physical laws* (for example, the law for freely falling bodies $s = \frac{1}{2} gt^2$); that is, they are supposed to describe adequately routines or patterns of physical experience. The final stage is the laboratory test of the law and its verification or refutation.

Clearly the operational viewpoint enters significantly on the *instrumental* level into two steps of the logical scheme just set forth: (i) in the definition of appropriate and useful instrumental constructs for describing physical experience, and (ii) in the laboratory testing of the consequences of the postulates of the theory expressed in terms of relationships connecting such constructs. The operational viewpoint enters on the "paper-and-pencil" level in the introduction of theoretical constructs as, for example, the velocity of any particular single molecule in kinetic theory, or the velocity of a single electron in atomic theory, or the state function (ψ function) in quantum mechanics. It also enters in the postulates or hypotheses of the theory which, though often suggested by experience, are essentially and in the last analysis free creations of the human mind.

It is just at this point that I should like to raise a question. Does the thoroughgoing operationalist (I dislike this term just as much as Bridgman does but see no simple alternative to its use in this context) require that before a physical theory can be considered a really satisfactory explanation of physical experience, instrumental interpretation or validation must be given to every element in the theory?

It seems to me that the whole history of physics exhibits the impracticability of this demand. If we examine critically the postulates of what are now called the classical physical theories, we find that even though the inventors may have wished to base their hypotheses more or less directly on experience and at times may indeed have thought that they were doing so, there nevertheless inheres an ideal element in all these postulates, not accessible to ex-

periment except through the special cases that result as logical deductions of the general principles. I suppose we all admit that Galileo took a common-sense approach to the problem of motion. At least it seems common sense to us today, although it did not seem so to most of his contemporaries. Yet his boldness in constructing out of his head the concepts of instantaneous velocity and acceleration, and showing that certain simple assumptions connected with them lead by direct mathematical deduction to the observed laws of falling bodies, justifies his position as the creator of theoretical physics. In more recent times physicists have not hesitated to call on their creative imagination for constructs like, say, Planck's quantum of action that have no direct connection with experience but whose unifying influence on large domains of physical phenomena has been considered ample justification for their postulation.

Now possibly this procedure of free imaginative construction has not been good physical methodology, but most physicists admit that it has worked pretty well, and the fact that the concepts have in most cases been of the paper-and-pencil variety has not prevented their successful employment. In this process there has admittedly been little assurance of the association of *truth* in the philosophic sense with physical concepts and postulates, but I think physicists have learned to eschew this rather illusive goal in favor of the more pragmatic one of *success* in subsuming large areas of experience and in predicting hitherto unobserved phenomena. But we should not forget that there are other criterions of the value of physical theories not to be despised, among them, for example, simplicity of formulation, esthetic appeal, and teachability to others. These have been sufficiently stressed elsewhere to need no further elaboration here.

I am puzzled by the phrase "although perhaps indirectly" in the quotation I just made from Bridgman. Perhaps this is the nub of my difficulty. If indeed by indirect connection of the postulates with experience one means simply that their logically deduced consequences can be instrumentally verified, then there is no problem; but somehow I find it hard to believe that this is what Bridgman means, since in a later part of the same paper he takes some pains to analyze and compare the action-at-a-distance (particle) and the field constructs from the standpoint of their instrumental connotations. Concerning himself with the rather well-known fact that both constructs can be made to lead to similar consequences in problems in mechanics and electricity, he reaches the conclusion that "there is no way by which the desired distinction between action at a distance and action by a field can be given instrumental significance." I cannot imagine that he would have devoted so much time to this discussion had he not attached significance to the desirable possibility of distinguishing between the two theoretical points of view by direct instrumental means. The inability to produce this distinction does not make me so unhappy as it apparently does the holder of the thoroughgoing operational point of view, since it seems to me that in the development of physics we must be constantly on the watch for different constructs and postulational systems. In fact, the continual creation of physical experience that is going on in physical laboratories renders this imperative, if the job of physical explanation is to keep pace with physical discovery.

One wonders whether the desire of the operationalist to subject the postulational structure of physical theories to direct instrumental test may reflect a conscious or unconscious feeling on his part that there exists some ultimately valid and verified explanation of all physical experience or, to put it in words often used, although to me they are meaningless, that there exists an ultimately *true* representation of *reality*? Probably I am an incorrigible conventionalist, but to me this viewpoint is illusory and has little to do with the success of physics as a science. If it really helped us forward in the search for better explanations, as a pragmatist I should not hesitate to endorse it enthusiastically. Unfortunately I can find no warrant for this belief. To me the future of physical theory lies in the bold use of imagination. No one knows whence the successful ideas will come, but no one can reasonably doubt that they *will* come in the future as they have in the past.

References

1. "The nature of some of our physical concepts," *Brit. J. for the Philosophy of Science* **1**, 257 (1951).
2. *The Nature of Physical Reality* (McGraw-Hill, New York, 1950), pp. 220 ff.

Remarks on the Present State of Operationalism

P. W. BRIDGMAN

Dr. Bridgman, who recently retired as professor of physics at Harvard University, is author of an article entitled "Science and common sense" that appeared in the July issue of The Scientific Monthly.

THERE would seem to be no reason why I am better fitted than anyone else to open this discussion. As I listened to the papers I felt that I have only a historical connection with this thing called "operationalism." In short, I feel that I have created a Frankenstein, which has certainly got away from me. I abhor the word *operationalism* or *operationism,* which seems to imply a dogma, or at least a thesis of some kind. The thing I have envisaged is too simple to be dignified by so pretentious a name; rather, it is an attitude or point of view generated by continued practice of operational analysis. So far as any dogma is involved here at all, it is merely the conviction that it is better, because it takes us further, to analyze into doings or happenings rather than into objects or entities.

What I conceive to be involved here may be a little clearer if the historical background is understood, and I hope you will pardon me if I interject some personal remarks. The date usually associated with this is 1927, the year of the publication of my book *The Logic of Modern Physics,* but preparation for this in my own thinking went back at least to 1914, when the task of giving two advanced courses in electrodynamics was suddenly thrust upon me. Included in these courses was material from the restricted theory of relativity. The underlying conceptual situation in this whole area seemed very obscure to me and caused me much intellectual distress, which I tried to alleviate as best I could. Another cause of distress was the situation in dimensional analysis, which at that time was often so expounded as to raise doubt whether experimental work was really necessary at all. The dimensional situation proved comparatively simple, and I was able to think the situation through to my own satisfaction—an experience that perceptibly increased my intellectual morale. The analysis, which was essentially operational, although the word was not used, was published in 1922 (*Dimensional Analysis,* Yale Univ. Press). I think the word *operation* was first explicitly used in a discussion that I gave at the Boston meeting of the AAAS in 1923 at a symposium on relativity theory participated in by George Birkhoff, Harlow Shapley, and myself.

The Logic of Modern Physics was written during a half sabbatical in 1926 under a stringent time limit, for I knew that at the end of September my laboratory would reabsorb me. In view of this time limit, I had to map out the questions that to me appeared most pressing and to be satisified with discussions of which I could say "at least this much must be true and be part of the final picture," and not attempt the more ambitious program of a complete analysis. In short, I was compelled to be satisfied with a "necessary" as opposed to a "sufficient" analysis. A great many interesting and important leads had to be left unexplored: for example, an analysis of what it is that makes an operation suitable for the formulation of a scientific concept; again, in what terms can operations be specified. It has, in fact, been a surprise to me that, since the publication of my book, so much of the concern of others has been with abstract methodological questions suggested by the endeavor to erect some sort of a philosophic system rather than with attempts to follow the more concrete and obvious leads.

Since writing the book, I have never again been able to devote as sustained attention to this field but have had to content myself with shorter excursions, resulting in a number of articles and a couple of thin books. But at the same time, with the continued practice of operational analysis, my ideas have been changing and growing and gaining in generality. If I were to start today to expound my attitude systematically, the order of presentation would be different. The general points of view would be presented earlier in the treatment, with, I think, avoidance of much confusion. It is often thought that there is a normative aspect to "operationalism," which is understood as the dogma that definitions *should* be formulated in terms of operations. As I see it, there is in the *general* point of view nothing normative whatever. An operational analysis is always possible, that is, an analysis into what was done or what happened. An operational analysis can be given of the most obscurely metaphysical definition, such as Newton's definition of absolute time as that which flows by itself uniformly and equably. What is more, any person can make an operational analysis, whether or not

he accepts what he supposes to be the thesis of "operationalism," and whether or not he thinks he is wasting his time in so doing. So far as the "operationalist" is to be distinguished from the "nonoperationalist," it is in the conviction of the former that it is often profitable and clarifying to make an operational analysis, and also, I suspect, in his private feeling that often the "nonoperationalist" does not want to make an operational analysis through fear that it might result in a change in his attitude.

If one has consistently used operational analysis, I think one's general point of view comes to acquire a certain flavor and certain considerations come to be emphasized in his thinking; these I shall endeavor to characterize briefly. In the first place, one is impressed by the observation that operational analysis can always be pushed to the point where sharpness disappears. The "yes or no" signal of recent information theory, the "all or none" firing of a neurone of the physiologist, and so on, lose their sharpness when considered as processes occurring in time, and the operations of logic lose their sharpness when the analysis is pushed to the point of self-doubt. Again, one is impressed by the complexity of the verbal structure that mankind has erected through the ages. Here is an autonomous world in which a man can, and frequently does, live a more or less self-contained and independent existence. On the other hand, despite the complexity of the verbal world, the external world of objects and happenings is inconceivably more complex—so complex that all aspects of it can never be reproduced by any verbal structure. Even in physics this is not sufficiently appreciated, as is shown, for example, by the reification of energy. The totality of situations covered by various aspects of the energy concept is too complex to be reproduced by any simple verbal device. As a corollary of the continued interplay of the verbal and the "objective" worlds, I personally have come to feel the value of analyzing our operations as far as possible into their "instrumental" and "paper-and-pencil" components and think there is much here that is still unexplored. I think there is much to be done in nonscientific fields along these lines. For instance, I anticipate that many of the operations of philosophy will be found to be essentially verbal and incapable of being made to emerge into the instrumental world. I believe that revolutionary results will follow a full realization of the inescapability and immanence of the element of human enterprise.

Turning now to a consideration of a couple of the points raised by the preceding papers, I am not particularly disturbed by the fact that it is sometimes difficult to fit the apparent demands of "operationalism" into a logically complete and satisfactory scheme. Part of this failure I think arises from misconception of what is involved; but in any event, as I have already intimated, I would expect that the analysis could be pushed so far that it would become unsatisfactory logically. That this should be possible appears to me to be fully as much a commentary on the nature of logic as on the nature of "operationalism." At the same time, I fully agree with Hempel that there is much unnecessary vagueness in such matters, as for example, in the answer to the question of what it is that makes an operation "good" for the purposes of the scientist. There is certainly much room for improvement here, and I think the improvement will be naturally forthcoming when the operational point of view has reached a higher state of development than at present.

With regard to Lindsay's question concerning my meaning in saying that it is desirable that the paper-and-pencil operations of the theorist be capable of eventual contact, *although perhaps indirectly,* with instrumental operations, I shall answer by giving two examples. The first is concerned with the stress at any interior point of a solid body exposed to external forces. This stress is a complex of six components, constructed by the theoretical physicist and incapable of measurement by any instrument, if for no other reason than that the interior points of a solid body are inaccessible. However, the stress is connected through the equations of elasticity theory with the forces acting upon the free faces, and these forces have immediate instrumental significance. Here, what I meant by an "indirect" connection is the connection through the equations of elasticity. Again, the psi function of wave mechanics, defined as a probability amplitude, is at first a pure construction of the theoretical physicist, but again it makes connection through mathematical operations, in this case operations of integration, with the mean density of electric charge, which does have instrumental significance.

With regard to my concern to show that there can be no instrumental distinction between action at a distance and action through a field, I did not feel badly about the discovery, as Lindsay inferred from my, I fear, obscure exposition. On the contrary, I felt much pleased with myself, because my reading of scientific literature had led me to suppose that most physicists assume that there is some essential "physical" difference between these two points of view. In showing that this distinc-

tion is on the "paper-and-pencil" level, I thought that I was really saying something. In general, I think that there need be no qualms that the operational point of view will ever place the slightest restriction on the freedom of the theoretical physicist to explore the consequences of any free mental construction that he is ingenious enough to make. It must be remembered that the operational point of view suggested itself from observation of physicists in action.

Beyond Operationalism

RAYMOND J. SEEGER

Prior to World War II, Dr. Seeger taught college physics and wrote textbooks on cultural physics. During the war he became interested in fundamental explosives research, especially shock-wave phenomena and afterward continued research in aerophysical phenomena, particularly hyperballistics. He was formerly chief of the Aeroballistic Research Department, Naval Ordnance Laboratory, White Oak, Maryland, and director, Institute for Fluid Dynamics and Applied Mathematics, University of Maryland. He is now (acting) assistant director for the Mathematical, Physical and Engineering Sciences Division of the National Science Foundation. He is also secretary of AAAS Section L (History and Philosophy of Science).

THESE remarks are concerned specifically with R. B. Lindsay's present paper on "Operationalism in physics reassessed" and more generally with P. W. Bridgman's 1950 lectures on the "Nature of some of our physical concepts."

As a fellow-student of Henry Margenau in a Yale class under Lindsay, I learned long ago to appreciate the economic clarification of physical concepts revealed by the positivist's (Mach, *1*) point of view. At the same time, however, I was introduced to the creative value of the conventionalist's (Poincaré, *2*) outlook. The customary interplay, however, of irreducible data and of thoughtful theory is itself a third factor that must not be ignored. Without some heuristic presuppositions I find that consistent operationalism is not pregnant theoretically, and without some faithful generalizations I find that thoroughgoing operationalism is not satisfactory intellectually. For me it has primarily pragmatic, but necessarily incomplete, values. As Planck (*3*) has emphasized, the laws of nature are not solely the inventions of man. In man's formulation of them, however, there is always something of himself; man's imprint is upon nature—but nature is there first.

The day before Christmas my little girl asked, "Daddy, do you believe in Santa Claus?" Restless the Christmas Eve sleep of a Scrooge who does not believe in Santa Claus! As we grow older a more meaningful question emerges: "What do you believe about Santa Claus, that jovial spirit of Christmas?" So, too, one may start initially with the immature question: "Do you believe in the operational point of view in physics? in operationalism, in general?" Inevitably, one is forced later to consider more precisely what one does believe. Granted that the final statements of physical theory must be operational in character in order that they may be checked with natural phenomena, what about the initial axioms? Can first principles, indeed, conceivably be operational, say, the inertial principle of classical mechanics or the state ψ functions of quantum mechanics? Then, too, what about intermediate statements? Must each step of a deductive approach itself be term-wise operational? If not, where does one draw the line between theoretical statements that are operational and those that are not? If there is a criterion, must it, too, be operational? It seems to me that not every operational statement is necessarily meaningful and that not every nonoperational statement is necessarily meaningless. For example, a conception like a chimera, may have operational components but no composite meaning.

We have been socially urged to speak "the truth, the whole truth, and nothing but the truth." A scientific statement, however, may be true to certain observed facts, but not true to others. For example, someone remarks, "He was a lion in the fight." Is this statement true? As far as a certain quality describing participation in the fight is con-

cerned, yes! Yet no one is so misled by the metaphor as to picture a real lion in the ring. On the contrary, progress in science has often been facilitated by the use of an analogy. For example, despite general acceptance of the kinetic theory of matter, physicists still find the concept of a mechanical continuum useful in describing the physical properties of fluids. In the case of analogy we may even form Grecian monsters, like all-seeing Argus, to emphasize certain characteristics. In this respect the image is true to certain aspects, but it does not portray nothing but the truth. Herein lies the danger! We may take our picture too seriously—we may persuade ourselves that the whole is true. A critical operational viewpoint would rightly prevent any such false impressions.

At the same time, however, by its minimal principle of economy, dogmatic operationalism may curb enthusiastic creativity. It places a premium upon economic description (Kirchhoff, *4*) without insuring imaginative prediction (Hertz, *5*), not to mention esthetic values. The former favors a completed theory; the latter, an evolving one. In the incipient stages of development, adequacy of expression may be more important than economy of thought. You ask me: "What is *x*?" Usually, I reply: "It is like this—but it is not exactly this." "It is like that—but it is not precisely that." (In this connection, recall the early interpretations of the unknown x-rays.) Simplicity as the primary criterion for systematizing data may be misleading if the data are incomplete. A classical example of this reactionary spirit is Mach's own antipathy to atomism with its sterile outcome.

There is no *a priori* reason, moreover, for expecting that the best form of descriptive theory can simultaneously provide predictability. For example, the esthetic appearance of a completed building may no longer reveal the crude outline of a simple scaffolding used in the construction. Thus, the simple theory of the Bohr atom might have been missed if one had to formulate first the more elaborate theory of quantum mechanics to comprehend not only the Balmer spectral lines but also multiplet and hyperfine structure, the Zeeman and the Stark effects. Too many details may fog the main outline. The finalistic operational world-view, indeed, may not be the best possible theoretical world in the making. A less logical picture, or even several possible world-constructs may be more heuristically valuable.

One further note of caution about dogmatism with respect to operationalism! It is conceivable that the developing world-picture, which we find to be increasingly true to observed data and believe to be made somewhat in our own intellectual image, may be real (in the everyday usage of that word). However arbitrary the description by an observer who cannot be completely detached from a phenomenon, there is an ever-growing body of physical measurements that can be reproduced independently of any one observer. We become more understandingly aware of the workings of a world outside ourselves, even though we may never attain direct and complete understanding of it. The relativism of our views may only distort an abosolute object. Science, I believe, is more than a grinning (mocking) Cheshire cat.

My underlining of Lindsay's chief criticism of thoroughgoing operationalism is best summed up, I believe, in the concluding (appendix) statement of Max Born (*6*): "Faith, imagination, and intuition are decisive factors in the progress of science as in any other human activity." We must go beyond operationalism! At the same time, however, despite Lindsay's own preference, I personally hope that we can go beyond conventionalism!

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No one can be really master of any science unless he studies its special history, which again is bound up with the general history of humanity.—A. Comte.

Operationism and Relativity

ADOLF GRÜNBAUM

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AGAIN and again P. W. Bridgman has told us that the restricted theory of relativity was at once the foundation and the inspiration of his espousal of the operational point of view (*1*). Since earlier critiques of operationism by other authors seem not to have contested Bridgman's invocation of relativity theory, I wish to explain here (i) why I cannot accept his philosophic interpretation of the restricted theory of relativity, and (ii) why I maintain that he is not entitled to rely on Einstein's analysis of space and time for support of his views.

Before considering relativity, it is necessary to articulate operationism in terms of distinctions that are familiar from the general theory of signs. Students of semiotic have found that it is both valid and important to distinguish between pragmatics and semantics when giving an account of an interpreted system of signs such as physical theory. It is admittedly illuminating to study psychologically, sociologically, and otherwise the activities of scientists in their pursuit of science and to investigate the relationships between scientific symbols and their human users. Studies of this kind constitute the field of pragmatics. But, from the standpoint of the *logic* of physics, our concern must be *not* with pragmatics but rather with the relationships between physical theory on the one hand and the designata or denotata of that theory in the realm of nature on the other. In this sense then, the logic of physics is the semantics of physical theory. Now, it seems to me that, fundamentally, Bridgman's thesis is that the distinction between pragmatics and semantics is illegitimate and unfounded, semantics being properly a mere part of pragmatics. For he writes (*2*):

> What now is the criterion that the meaning of a term in isolation is known? Here again [that is, just as in the case of effective communications, taken as a whole] the criterion can be formulated in terms of action. If I know what it was that my fellow did to decide to use the term, or if my fellow can reconstruct for himself what it was that I did in deciding to use the term, then, I think we must say that the meaning of the term in that particular usage is fixed. . . . We see, therefore, that a specification of meanings, both of isolated terms and of communication in toto, involves a specification of action of some sort.

No one will wish to deny that a study of the activities of scientists should comprise their work behavior in the laboratory as well as their work with mathematical and linguistic symbols. But the objects that physics talks about are neither the manual nor the conceptual activities of physicists; instead, physics is concerned with the postulational and observational ascertainment of the attributes and relationships that characterize various kinds of physical entities and processes. These latter entities and processes are the objects or designata of both classical and quantum physics. For, as H. Reichenbach has justly pointed out (*3*):

> Like all other parts of physics, quantum mechanics deals with nothing but relations between physical things; all its statements can be made without reference to an observer. The disturbance by the means of observation—which is certainly one of the basic facts asserted in quantum mechanics—is an entirely physical affair which does not include any reference to effects emanating from human beings as observers. [p. 15] . . . The instrument of measurement disturbs, not because it is an instrument used by human observers, but because it is a physical thing like all other physical things [p. 17].

Accordingly, I do not see how Bridgman can escape the charge that he is gratuitously absorbing semantics within pragmatics. Once we recognize, however, that he regards this absorption as fully justified, we can readily understand why he writes as follows (*4*):

> The presence of an operational component in any analysis of concepts seems to me to be a simple result of observation and not to be a matter for argument. It is doubtless more usual to attempt an analysis in terms of objects of the external world or in terms of fixed and static elements of other sorts. But when I ask myself what I mean when I say that objects exist, or that there is an external world, the only answer that I can give is in terms of activities of one sort or another.

Furthermore, we can now see why he is led to give a psychologistic account of the logical nerve of deductive inference, an account that implicitly denies

Frege's distinction between the context of justification and the context of discovery (*5*). And it is therefore not at all surprising that Bridgman's analysis of the restricted theory of relativity will exhibit the defects of his thesis with particular clarity, as I shall now endeavor to show.

The following are the results of the relativistic space-time analysis pertaining to the issue before us: any two physical events, whether we happen to observe them or not, sustain the objective temporal relationship of "later" or "earlier," if it is physically possible that they be the termini of a causal influence chain. I deliberately do not speak of connectibility via signal transmission, since such a formulation may suggest misleadingly that essential reference is made to some human dispatcher of the signal who wishes to use the signal as a means of communication with other human beings.

Mehlberg, Zilsel, and Denbigh have demonstrated convincingly (*6*) that this causal theory of time does *not* succeed in providing a criterion for the unidirectionality of time, but I have shown elsewhere (*6*) that the required criterion does *not* involve recourse to man's subjective sense of time-direction or to human activities, as Bridgman claims (*6*).

The relevant crucial difference between the Newtonian and relativistic accounts of time derives from the following fact: although pre-Einstein physics had postulated the existence of arbitrarily fast causal chains, including some of infinite velocity, the experimental work of W. Kaufmann and others persuaded Einstein to postulate that electromagnetic processes are the fastest causal chains in nature. Accordingly, in the Newtonian theory, every two events of nature stand in a determinate, unambiguous temporal relationship to each other: either they can be the termini only of causal chains of *infinite* velocity, as in the case of gravitational action-at-a-distance, or they will be the termini of causal chains of finite velocity, however large. In the former case they are absolutely simultaneous and in the latter absolutely nonsimultaneous. In restricted relativity, on the other hand, no influences can propagate themselves faster than light, and therefore every class of physical events in which each member sustains an objective relationship of temporal order to every other member will be only a proper subclass of the totality of physical events. There will be pairs of physical events whose members cannot be the termini of any influence chain and, therefore, are not related temporally by any objective criterion. But clearly the relationships between the events that *are* the termini of physically possible influence chains are *not* generated by the operations performed by human beings and do not depend in any way upon the activities of human beings or upon their presence in the cosmos. The human element does not enter until a symbolic description of these objective time relationships is given—that is, until we assign numerical *names* to these events. This assignment of names is made in such a way that the names or (time)-coordinates are indicative of the objective (temporal) relationships between the events in question as well as of certain conventions. In the case of the events that are *not* temporally related objectively, there is no nomological restriction on the names or time-coordinates to be assigned, and then the description can be governed by considerations of mathematical convenience. In particular, here is the source of the relativity of distant simultaneity and of the need for introducing a stipulation as to which particular pairs of such events will be called "simultaneous." Whatever may have been Einstein's conceptual orientation when he first made his analysis of distant simultaneity, the systematic import of that analysis relevant here is that nature permits greater latitude for the consistent assignment of numerical *names* to physical events than was thought permissible in classical physics. But apart from this somewhat greater freedom of naming, the relativistic temporal order of nature neither is generated by, nor derives its meaning from, *our* hypothetical or actual signaling activities or from any other operations performed by human beings.

Thus, the upshot of Einstein's analysis concerning the issue before us is *not*, as Bridgman would have it, that the concepts of science refer to our operations instead of to the properties and relationships of physical events (*7*). What the restricted theory of relativity *does* teach us, however, is that the properties and relationships of physical events and things are different in several important respects from the ones that Newton had postulated. As Max Born has justly remarked in his recent paper "Physical reality" (*8*):

> . . . the theory of relativity . . . has never abandoned all attempts to assign properties to matter, but has refined the method of doing so in order to conform with certain new experiences. . . . [The] root of the matter is a very simple logical distinction which seems to be obvious to anybody not biased by a solipsistic metaphysics; namely this: that often a measurable quantity is not a property of a thing, but a property of its relation to other things. . . . Most measurements in physics are not directly concerned with the things which interest us, but with some kind of projection, this word taken in the widest possible sense.

In particular, if natural clocks happen to be synchronized via light in the manner of Einstein's definition and if material rods are copresent with such clocks in the various Galilean frames, then these physical recording devices will show the readings required by the Lorentz transformations quite apart from any conscious human observer or "operator" (*9*), whereas Bridgman tells us (*10*) that

> If the meanings of science are to be found in operations, then there must be a performer of the operations and this is of necessity a human performer.

In fact, as he himself recognizes, his meaning postulate, coupled with the privacy of sense data, makes operationism fundamentally incompatible with the basic idea of relativity, which is the idea of *invariance* with respect to reference frames (*11*).

It should be noted that, although Bridgman does not deny the feasibility of a description of nature in terms of properties and relationships, he claims certain very definite advantages for a description based entirely on operations (*12*). What are these advantages and on what grounds does he claim them? He writes (*13*):

> What we are in effect doing in thus preferring the operational attack is to say what we *do* in meeting new physical situations has a greater stability than the situations themselves and that we can go further without revising our operations than we can without revising our picture of the properties of objects.

He attempts to illustrate this claim by specific reference to the history of relativity and says (*14*):

> Reflection on the situation after the event shows that it should not have needed the new experimental facts which led to relativity to convince us of the inadequacy of our previous concepts, but that a sufficiently shrewd analysis should have prepared us for at least the possibility of what Einstein did. . . . We should now make it our business to understand so thoroughly the character of our permanent mental relations to nature that another change in our attitude, such as that due to Einstein, shall be forever impossible.

And he adds that the only way of making sure that we can "render unnecessary the services of the unborn Einsteins" (*15*) is to refuse to link the same concept to a variety of operations, since this nonunique operational anchorage of concepts "subjects us to the constant danger that we may get different numbers by these different operations when experimental accuracy is improved" (*16*). I do not think that this thesis will bear examination.

Let us see whether careful attention to the operational anchorage of our concepts could have forestalled or lessened the surprises and revisions that were made necessary by relativity. Suppose that a unique operational procedure had been insisted on in classical physics for the specification of distances and that the same had been done for time determinations. Would that have forestalled the Newtonian assumptions of absolute simultaneity and of the nondependence of mass on velocity, thereby cushioning us against the surprising results of Kaufmann's experiments? To be sure, it would have done so in the sense that we would not have measured any group velocities exceeding that of light and we would then not have assumed the existence of signals of arbitrarily large velocities. Neither would we have felt that mass is always independent of velocity. But note the prohibitive price that would have had to be paid for this spurious gain: Newton's law of universal gravitation and third law of motion with their instantaneous action-at-a-distance could not have been enunciated. Neither could Newton have stated his second law of motion, for it tells us that a material particle can be brought to an arbitrarily large velocity by a sufficiently large force acting for a sufficiently long time, if the mass is assumed constant regardless of the velocity. In short, the price of the *safety* that could have been attained by operationist caution and restraint would have been, among other things, the immensely fruitful system of classical celestial mechanics! In fact, if the rule of operationist caution is strictly and consistently applied, physics must reduce to a mere record of isolated data, since the criterion does not warrant making extrapolations in *any* direction. In particular, how, in the absence of the information yielded by the experiments of the years between 1900 and 1905, could a *safe* yet *nonsterile* theory of mechanics have been constructed at all? And how could attention to unique operational anchorage of concepts have immunized classical optics against the results of the Michelson-Morley experiment without first rendering that theoretical discipline almost totally impotent? It is quite true that operational awareness would have made us less surprised to find that the length of a moving body is different from the corresponding rest length of that body and less prone to make the unconscious *a priori* assumption that these lengths are equal. But awareness of the inductive leaps involved in the postulational and extrapolative ascription of *properties* and *relationships* to physical things would have secured the same advantages for us without sacrificing the immense theoretical fruitfulness achievable through a description in terms of properties. Thus, in the case of length, it is not attention to operations of

measurement but Cantor's *theory,* formulated in terms of properties and relationships, that tells us that we cannot determine the length of a body *AB* on the basis of the number of points between *A* and *B*, because there are just as many points in a short segment as in a long one. When this result is coupled with the purely theoretically discovered Riemannian theory of manifolds, it becomes clear that the length of body *AB* cannot be an attribute of the space between the points *A* and *B* or a relationship between these points but must be an attribute of the relationship between the body *AB* on the one hand and the standard of congruence on the other.

An analogous consideration can be advanced against the operationist's invocation of the principle of equivalence of the *general* theory of relativity, since this principle lends itself to formulation both in terms of properties and in terms of theoretical operations. In all these cases, a description in terms of properties and relationships yields all the advantages secured by an operational description. But a description based on properties also secures for us a range of theoretical fruitfulness that no genuine operational description could ever hope to achieve. In fact, it is a description based on properties that explains, among other things, why it is that diverse operations do *or* do not yield the same numerical results under certain conditions and tells us how to enlarge our operational horizons through new experiments (*17*).

It seems inevitable, therefore, that the price of fruitfulness is a description in terms of properties and relationships that entails the risk of requiring the services of unborn Einsteins! This is not to deny that *historically,* a kind of operational critique contributed to the abandonment of H. A. Lorentz's form of the ether hypothesis, a hypothesis whose vacuity becomes equally apparent, however, upon asking what *properties* and *relationships* are intelligibly ascribed by that hypothesis to physical objects and events.

Thus, as I see it, operationism can contribute significantly to our knowledge, if it is construed as part of the restricted discipline of pragmatics but *not* if it is interpreted as an account of the logic or semantics of physics.

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7. See *OAM*, pp. 255–256; *NPT*, pp. 9–10; and *LMP*, pp. 6–9.
8. M. Born, *Phil. Quarterly* 3, 143 (1953).
9. H. Reichenbach, *Axiomatik der relativistischen Raum-Zeit-Lehre* (Vieweg, Braunschweig, 1924), p. 70.
10. *RPVP*, p. 18. This meaning postulate leads Bridgman to make a curious aprioristic pronouncement in regard to concepts that will prove fruitful in social theory, for he thinks that the postulate guarantees that *"The individual is the unit in terms of which all our social concepts ultimately find their meanings"* (*ibid.*, p. 20).
11. P. W. Bridgman, "Einstein's theories and the operational point of view," in P. A. Schilpp, Ed., *Albert Einstein: Philosopher-Scientist,* Library of Living Philosophers, vol. VII (Evanston, Ill., 1949), pp. 335–354, and Einstein's reply, *ibid.*, p. 679; *RPVP*, p. 19.
12. See *NPT*, pp. 9–10.
13. *OAM*, p. 257.
14. *LMP*, pp. 1–2.
15. *LMP*, p. 24.
16. *OAM*, p. 255. See also *RPVP*, p. 3, but note Einstein's own contrary attitude on p. 193 of his "Geometry and experience," as reprinted in Feigl and Brodbeck, Eds., *Readings in the Philosophy of Science* (Appleton-Century-Crofts, New York, 1953).
17. This point is illustrated with respect to the concept of pressure by R. B. Lindsay in "A critique of operationalism in physics," *Phil. Science* 4, 458 (1937).

Should Fluorides Be Added to Public Water Supplies? 79

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Intro

ALTHOUGH dental diseases rarely cause death, their costs in money, time and pain are enormous. In the United States during the year that ended 1 July 1953, a total of 1.6 billion dollars is reported to have been spent for various forms of dental care, including the repair and replacement of teeth damaged or lost through tooth decay and diseases of the gums (*1*). This amount equals 15.7 percent of the total national health bill of 10.2 billion dollars. Unquestionably this expenditure for dental treatment far exceeds the financial cost of any other single health problem among American citizens. Still these figures do not reveal the true magnitude of dental disease in the United States. In the same survey, it was estimated that only 17 percent of persons in families with incomes less than $2000 sought dental treatment; even in families with incomes of $7500 or more, only 56 percent had any dental care during the year.

In the past few decades, many precise and esthetically pleasing procedures have been developed and effectively used for the restoration of tissues destroyed by dental disease. Despite all these procedures, there are indications that tooth decay is occurring more rapidly now than as recently as two decades ago (*2*). Today tooth decay is everybody's problem. Relatively few of our citizens reach maturity, much less old age, without one or more decayed teeth.

On the basis of our present knowledge about the prevention of tooth decay, only one proposal appears to have a tangible potentiality for appreciably reducing the amount of tooth decay on a nationwide basis. This proposal is to increase the fluoride content of public water supplies.

Is a beneficial nontoxic procedure to increase the fluoride content of public water supplies feasible? whether Probably no other question about our health has been debated more widely in recent months. The proponents believe that this method will decrease tooth decay among urban populations by at least 50 percent without any toxic complications. The opponents have questioned the benefits, the safety of the procedure, and the motives behind the recommendation. Throughout the United States and in other countries as well, earnest people in lay and professional groups have been seeking to evaluate the various pros and cons.

Basically the present discussions about the fluoridation of public water supplies center around five questions: (i) What are the fluorides and where are they found? (ii) Is the ingestion of fluorides beneficial? (iii) Is the prolonged ingestion of fluorides at low levels toxic? (iv) Can fluorides be accurately introduced into water supplies? (v) Can fluorides be used safely and effectively in any other way? Let us examine the scientifically substantiated *facts* that pertain to each of these questions.

What are the fluorides and where are they found? Fluorine is element No. 9 in the periodic table with an atomic weight of 19.00. It was not isolated in pure form until 1886, by Henri Moissan (*3*). It is the lightest and the most reactive element of the halogen series. Two other members of this series, chlorine and iodine, are known to be essential nutrients for animal life. Fluorine is never found in free form in nature because of its reactivity. Instead, it occurs in combination with the various positively charged elements in simple readily ionizable inorganic salts known as fluorides. Its compounds are common in the earth's crust, with fluorspar (calcium fluoride) and cryolite (sodium aluminum fluoride) being particularly plentiful. Our agricultural soils contain large amounts of the various compounds of this element. In a survey of 20 important agricultural soils in New Jersey, the fluoride content of the topsoil was found to vary from as little as 29 up to 409 lb/acre (*4*). It has been estimated that the entire crust of the earth contains an average of 0.1 percent of fluorides (*5*).

Plant and animal tissues invariably contain detectable amounts of fluorides. As early as 1805, Morichini recognized that the fluorides were components of fossil teeth (*6*). Extensive surveys of the fluoride content of more than 130 foods are available. The majority of foods such as vegetables, meats, cereals, and fruits contain 0.2 to 0.3 parts per million (ppm) of fluorides. Outstanding exceptions to this lower range are the sea foods, the edible portions of which contain 5 to 15 ppm of fluoride, and tea leaves, which contain 75 to 100 ppm. A cup of tea will supply approximately 0.12 mg of fluoride. Reliable analyses of the fluoride contribution by foods from such far separated areas as Toronto, Minneapolis, and Washington, D.C. indicate that the average diet supplies anywhere from 0.18 to 0.56 mg of fluoride daily without the use of unusual amounts of either sea foods or tea (*7–9*).

Interestingly enough the fluoride content of plant tissues seems to be characteristic of the species rather than of the fluoride content of the soil. The content of bones and teeth varies, depending upon the amount of fluorides ingested. The content of other animal tissues is either not affected, or is only slightly affected by the amount of fluorides consumed. Because of the omnipresence of fluorides in all foods from plant and animal sources, no laboratory diet for experimental animals has been devised that is completely devoid of fluorides. This must be done eventually in order to determine whether any abnormalities will occur in animals that are completely deprived of this element throughout growth, maintenance, and reproduction.

Is the ingestion of fluorides beneficial? As early as 1874, Erhardt (*10*) and then again in 1892, Crichton-Browne (*11*) suggested that fluorides were important in the maintenance of teeth. The latter believed that his contemporaries consumed too little fluorides. He based this hypothesis on the increasing tendency to consume white bread and other highly refined foods, which were lower in fluorides than whole grains. He strongly recommended reintroduction into the diet of appropriate amounts of fluorides. Little was done to examine the merit of these postulates. One of the first convincing evidences of such a relationship was provided by Bunting and coworkers in 1928, who reported the results of a survey in Minonk, Illinois (*12*). The amount of tooth decay in children born and reared in this community was much less than in children who moved to Minonk after tooth development was complete. At the time of the survey, the investigators recognized that this striking difference was related to the water supply, but the active agent was unknown. Later it was found that the drinking water contained 2.5 ppm of fluorides.

In 1939, more exact information was given by Dean and collaborators (*13*) as the result of a survey of 1581 children in four communities in Illinois where the water contained varying amounts of fluorides. Later, a more comprehensive survey was described for 4425 children from 13 cities in four states (*14*). The data from the latter study are presented in Table 1 in terms of the number of decayed, missing or filled permanent teeth, observed in 12- to 14-yr-old children. Where the water contained 1 ppm or more of fluorides during tooth development, the children had a much lower incidence of tooth decay than children in nearby communities where the water contained appreciably less than 1 ppm. These findings have been corroborated by investigators in other areas of the United States as well as in Canada, England, South Africa, the Ukraine, Italy, Greece, and Hungary. Deciduous teeth likewise have been shown to benefit when waters containing these amounts of fluorides were available during tooth development (*15*).

Not only are the deciduous and permanent teeth of children benefited by naturally occurring fluorides. Adults in the United States, Argentina, England, and Hungary who were exposed to fluorides during tooth development likewise have a low incidence of tooth decay (*16–19*). On the basis of these and many other studies, there can no longer be any doubt that the consumption of drinking

Table 1. A comparison of the fluoride content of the drinking water and the amount of tooth decay among 4425 children, 12 to 14 yr of age in 13 cities from 4 states; based on reference (*14*).

	Fluoride content (ppm)	No. of children examined	Children with no tooth decay (%)	Avg. no. of diseased teeth per child
Colorado Springs, Colo.	2.6	404	28.5	2.5
Galesburg, Ill.	1.9	273	27.8	2.4
East Moline, Ill.	1.2	152	20.4	3.0
Kewanee, Ill.	0.9	123	17.9	3.4
Pueblo, Colo.	.6	614	10.6	4.1
Marion, Ohio	.4	263	5.7	5.6
Lima, Ohio	.3	454	2.2	6.5
Middletown, Ohio	.2	370	1.9	7.0
Zanesville, Ohio	.2	459	2.6	7.3
Quincy, Ill.	.1	330	2.4	7.1
Portsmouth, Ohio	.1	469	1.3	7.7
Elkhart, Ind.	.1	278	1.4	8.2
Michigan City, Ind.	.1	236	0	10.4

water containing 1 ppm or more of naturally borne fluorides throughout the period of tooth development confers a significant and prolonged caries resistance.

The fluoride content of teeth developed in areas where different amounts of fluoride were present in the water closely parallels the amount in the water (*20*). Where the drinking water contained 0 to 0.3 ppm, as in Washington, D.C., the teeth of the native continuous residents had approximately 0.010 percent of fluorides in the enamel and 0.024 percent in the dentin. Where the water contained 1.0 to 1.2 ppm of naturally occurring fluorides, as in Aurora, Illinois, the teeth of comparable residents contained 0.014 percent fluorides in the enamel and 0.036 percent in the dentin. Presumably the caries-resistance of the teeth is somehow related to their fluoride content.

Since the inorganic fluorides that occurred naturally in water supplies had proved to be so effective, the next step was to determine whether the introduction of comparable inorganic fluorides into low-fluoride waters would be of equal value. The first survey was begun at Grand Rapids, Michigan, in January 1945, where the fluoride content of the water supply was increased to 1.2 ppm under the joint sponsorship of the U.S. Public Health Service, the University of Michigan, and the Michigan State Department of Health. Muskegon served as the control low-fluoride city. Soon after this, surveys were begun at Southbury, Connecticut, where Mansfield served as the control city, and at Newburgh, New York, where Kingston served as the control city. A similar study was begun about the same time at Brantford, Ontario, with Sarnia as the control low-fluoride city; nearby Stratford served as a further control because of its naturally fluoride-bearing water supply. Somewhat later, Evanston, Illinois, and Sheboygan, Wisconsin, were selected as study areas. In all these communities, detailed evaluations were made of the incidence of tooth decay before fluoridation and periodically thereafter. Medical examinations of various kinds have been conducted in these areas during the same period to determine whether any systemic problems were caused by fluoridation.

Almost a decade has passed since the beginning of the Grand Rapids survey. Some of the impressive data that are now available from the older surveys are presented in Table 2 (*21*). The over-all analysis of these data unquestionably indicates that the dental caries incidence in teeth formed during the survey period was on the average about 50 percent lower than the caries incidence in otherwise comparable teeth formed prior to the increase in fluoride content of the water supply. As would be expected, the greatest benefits were in the younger groups. The similarity of the data from the several survey communities is amazing. No comparable reductions in dental caries incidence were noted in the children of the nearby cities where the fluoride content of the communal water supplies was not increased.

Is the prolonged ingestion of fluorides at low lev-

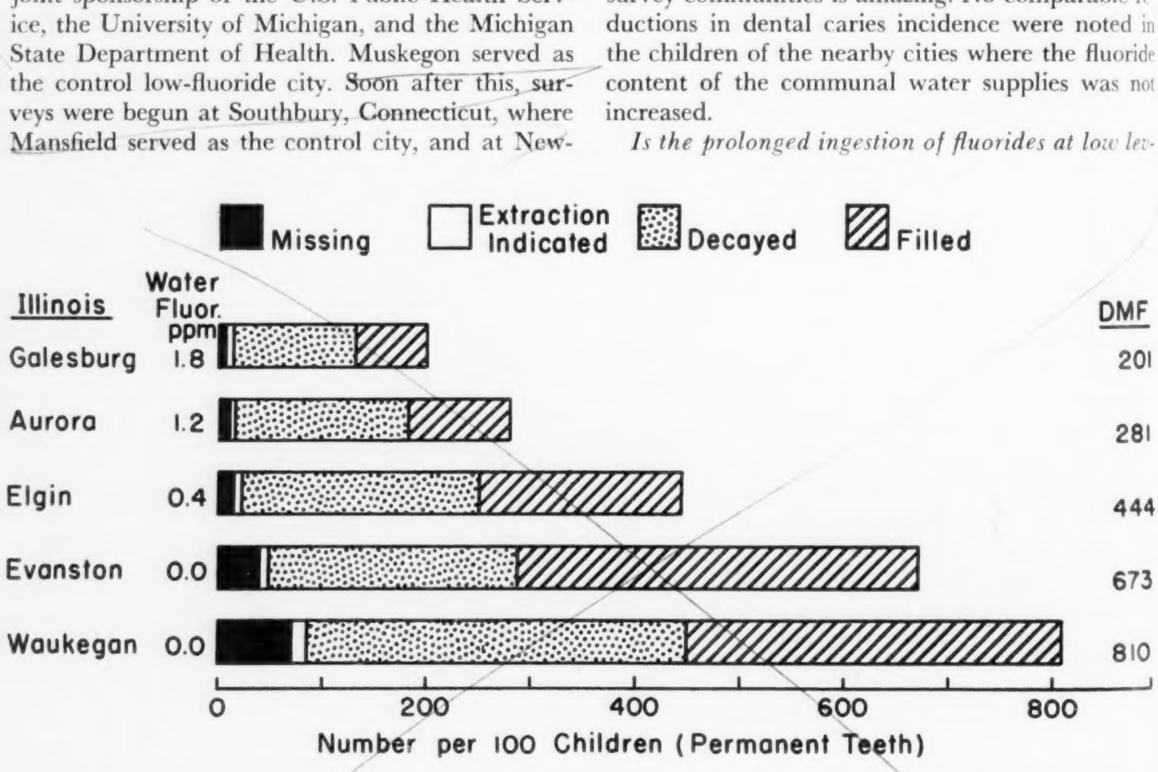

Fig. 1. Distribution of the four signs of dental caries experience in children 12–14 yr of age who have consumed varying amounts of naturally borne fluorides throughout life (*36*).

Table 2. Reduction in tooth decay observed in various fluoridation study projects; based on reference (*21*).

Community	Fluoridation: Date started	Fluoridation: Report period (yr)	Age group (yr)	Reduction in decay* (%)
Grand Rapids, Mich.	Jan. 1945	8	6	70.8
			7	52.5
			8	49.2
			9	48.1
			13	39.7
Brantford, Ont.	June 1945	7	6	59.4
			7	69.5
			8	51.5
			9	46.2
			13	32.9
Newburgh, N.Y.	May 1945	7	6	69.4
			7	67.8
			8	40.4
			9	51.4
Evanston, Ill.	Feb. 1947	4	6	73.6
			7	56.4
			8	35.4
Sheboygan, Wis.	Feb. 1946	6	9–10 (4th grade)	35.3
			12–14 (8th grade)	29.7

* Decayed, missing, and filled teeth.

els toxic? in 1918, McKay, a dental practitioner in Colorado Springs, described an abnormality in the enamel of the inhabitants' teeth of that area (*22*). Instead of its normal lustrous translucent appearance, the enamel of the affected teeth had dull chalky white, irregularly distributed patches. This abnormality has become known in the United States as "mottled enamel." In more severe cases, there is a pitting of the enamel, and the affected regions become stained a characteristic brown. McKay commented on the lack of an increase in tooth decay in these disfigured teeth but evidently did not note their peculiar caries resistance.

This abnormality occurred only in individuals who were born in the community or who had established their residence there during early infancy. Among the children who were born elsewhere and then moved to Colorado Springs several years later, the enamel that calcified prior to the change of residence never had any evidence of mottling. The enamel of those teeth that calcified thereafter was mottled in a manner similar to that of the natives. McKay collected ample evidence that this abnormality was caused by some material in the water supply. In 1931 three separate laboratories published evidence indicating that excessive fluoride ingestion during tooth development was responsible for mottled enamel. Thus, fluorides first became of interest in the field of public health because of an abnormality in the enamel resulting from injury to the cells that form the enamel.

Thorough epidemiologic studies have been made to determine the amount and the frequency of mottling in the teeth of individuals who were reared in communities where the water contains varying amounts of fluorides. On the basis of large reliable surveys, approximately 90 percent of children reared in communities where the water contains approximately 1 ppm of fluorides have teeth that are free of any trace of developmental abnormality. Numerous investigators believe that the shadings and color tones in teeth developed in areas with 1 ppm of fluorides are esthetically more attractive than those of the teeth in low-fluoride areas. The remaining 10 percent of children have minute areas in the enamel of their teeth that have been described as mottled to a very questionable or mild degree (*23*). This amount of mottling was never more than barely detectable under ideal examining conditions by investigators who were thoroughly accustomed to the appraisal of the surface appearance of enamel. It was so small as to be undetectable by a layman under practically any circumstances and has been considered by all investigators to be completely insignificant insofar as appearance is concerned.

As the fluoride content of the water increased appreciably beyond 1 ppm, the percentage of individuals affected and the severity of the mottling increased, until at 8 to 10 ppm a high percentage of the individuals who grew up in the area had mottled enamel of such severity that it was esthetically disfiguring. At fluoride levels over 2.5 ppm, the water supply is in need of attention either by the development of a new source, by adequate dilution with low-fluoride waters, or by chemical removal of the fluorides.

On the basis of these epidemiologic surveys, fluorides contributing about 1 ppm of fluorine to a water supply have been considered to be insignificant with respect to mottled enamel. The actual amount of fluoride to be recommended for any community will vary depending upon climatic conditions. In northern areas of the United States and in Canada, the most suitable level appears to be about 1.2 ppm. In southern states where the average annual temperature is higher and water consumption is greater, 1.0 ppm probably is the highest value that is desirable.

Abnormalities other than those caused in the developing teeth by excess fluoride ingestion have

been sought in various investigations in the United States. In one survey, height, weight, and bone fragility were measured (*24*). These data are presented in Table 3. Young males from regions where there was 1 ppm or more of fluoride in the drinking water supply were of comparable height and weight and had had no more bone fractures during their life than comparable individuals from low-fluoride regions.

Probably the most important and extensive surveys about general systemic influences of fluoride ingestion were those made in 1943 and 1953 in Bartlett and Cameron, Texas (*25*). The former community had a water supply that contained approximately 8 ppm of fluoride, whereas the latter community, situated some 30 mi distant, had water with essentially no fluorides. In 1943, a series of inhabitants who had resided at least 15 yr in each community was selected at random and carefully examined by skilled physicians. Totals of 116 in Bartlett and 121 in Cameron were studied. These individuals ranged from 15 to 68 yr of age in 1943; 57.8 percent of the Bartlett participants and 47.2 percent of those from Cameron were over 55 yr of age. X-rays were made of their skeletal systems, and full case histories were taken. Except for those who died in the intervening decade, the same individuals were examined or were contacted again in 1953.

The data obtained in these surveys indicate that there was no significant difference in any phase of health between individuals in the one community and the other with two exceptions. Many of the individuals who had resided in Bartlett during childhood had severely mottled teeth. In addition, a slightly higher incidence of cardiovascular disease was observed in Cameron. In all other respects, there were no detectable abnormalities that could be attributed to the different fluoride content of these two water supplies.

Table 3. Comparison of the height, weight, and bone-fracture experience of 1458 high-school boys, ages 15 to 17, residing in cities with different concentrations of fluoride in public water supplies; based on reference (*24*).

	Fluoride (ppm)	Height (in.)	Weight (lb)	Total no. of bone fractures per 100 boys
Galesburg-Monmouth, Ill.	1.8	67.2	135.6	29.0
Aurora, Ill.	1.2	66.7	136.5	25.3
Elgin, Ill.	0.5	68.0	136.1	24.3
Quincy, Ill.	.1	67.2	134.2	21.3
Waukegan, Ill.	.0	67.4	135.8	25.0
Washington, D.C.	.0	68.4	140.0	32.4

Bartlett was included in this survey because its water supply contained about 8 times the amount of fluoride recommended for usage in any fluoridation project currently in operation or proposed. It is located in a hot region where relatively large amounts of water are consumed by the inhabitants. Since no abnormalities attributable to fluoride ingestion, other than mottled enamel, were observed among the citizens of Bartlett, this, together with all other available data, strongly demonstrates the safety for inhabitants of all ages to use water supplies containing 1 ppm of fluorides for prolonged periods.

Many damaging statements about the toxicity of fluorides have been made in the course of public discussions concerning the fluoridation of public water supplies. Increased cancer frequency, increased incidence of arthritis, depression of the level of intellect, and many other ailments have been mentioned, indeed stressed, as toxic manifestations attributable to fluoride ingestion at the 1-ppm level. I find no indication in the published literature of an increased occurrence of these or any other disease entities in areas of the United States where the water contains fluorides far in excess of the recommended level.

If this opinion were based on only one or even on a dozen communities where natural fluoride-bearing waters have been ingested for prolonged periods, there would be justification for postponing the fluoridation of public water supplies. However, an examination of the record indicates that more than 3 million individuals in the United States have consumed fluoride-bearing waters in excess of 1 ppm for decades and an additional 5 million individuals have consumed amounts between 0.5 and 1.0 ppm for long periods (*26*). The fact that there is no known increase in disease in these communities is dramatic evidence of the safety of this measure.

Can fluorides be accurately introduced into water supplies? The two compounds commonly recommended for the fluoridation of public water supplies are sodium fluoride and sodium fluorosilicate. Both compounds are inorganic, readily soluble, and ionize almost completely in aqueous solution. In these respects, they are identical to the compounds occurring in natural fluoride-bearing waters. Either compound can be introduced in accurate amounts into a public water supply by means of carefully designed machinery. The quality of the apparatus is comparable to that used for the routine introduction of various other compounds used to maintain

the high quality of drinking water (*27*). One type of equipment is designed to introduce a solution of either compound; another type is designed to introduce the solid directly into the water system. Both types are so designed that the amount of fluoride delivered is proportionate to the volume of the water flowing by the machine, and both are located where there is sufficient agitation to cause uniform dispersion.

The amount of fluoride in the water can be readily and accurately determined by any one of several reliable chemical methods (*28*). In addition, an electronic instrument has become available that can be located in such a position as to measure the electric conductivities of the water before and after the fluoride introduction (*29*). This device is calibrated and designed to provide a continuous and permanent record of the fluoride addition on the basis of the change in conductivity.

In addition, samples should be analyzed periodically in a state laboratory to determine how carefully fluorides are being introduced by any municipality. Through these procedures, the amount of fluoride introduced can be accurately controlled, and any variation, even though minute, can be detected quickly.

Can fluorides be used safely and effectively in any other way? Four or more meticulous applications of concentrated solutions of fluorides to the external surfaces of teeth in young children in a period of 4 to 6 wk have been shown to reduce the number of new cavities in the succeeding year by 35 to 40 percent (*30–32*). This level of protection decreases appreciably until there is little or no detectable effect by the third or fourth year after treatment. Hence the recommendation has been made that the topical applications be repeated every 3 yr. These figures obviously indicate that the consumption of water-bearing fluorides at optimum levels during tooth development can be conservatively stated to be 2 to 3 times more effective than the topical treatment. In addition, the topical treatments require much time on the part of a dentist or hygienist, and this immediately imposes a restriction on the number of children who can be benefited.

No other effective means has been found for applying fluorides externally to teeth after their development has been completed. Fluorides in tooth powders and mouthwashes have been tested in preliminary fashion with negative results (*32*). Numerous proposals have been made for the use of some food or food component as a vehicle for fluoride administration. Salt and milk have been

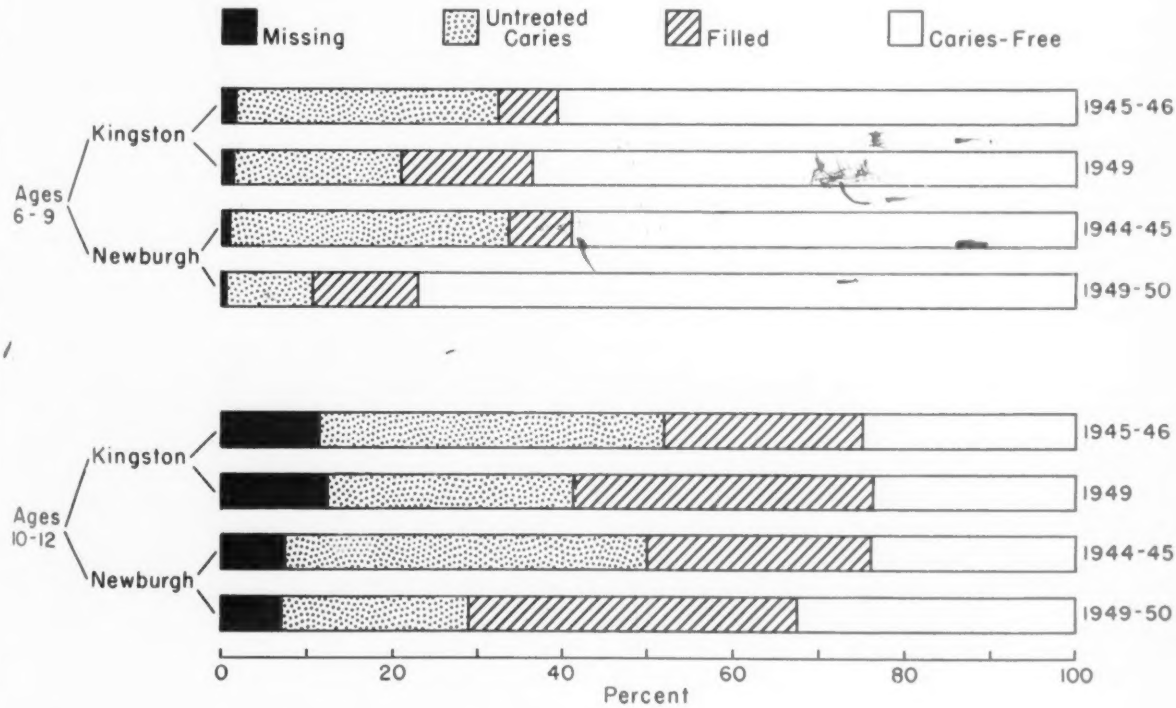

Fig. 2. Clinical status of erupted first permanent molars in children in Newburgh and Kingston, N.Y., 1944–50—that is, before beginning fluoridation and 6 yr thereafter (*36*).

frequently suggested. A common belief involved in these proposals is that such fluoride-enriched foods would be used only by the child population on a free selection basis. In the first place, these alternatives to water fluoridation have not undergone even a preliminary test for their effectiveness. This lack of knowledge is in appalling contrast to the detailed observations on the effects of water-borne fluorides. Second, there are no well-substantiated data on the variations in individual consumption of such materials as salt and milk. Even though the fluorides made available through either food might be as effective as water-borne fluorides, the individual variation in consumption may be so wide that it would be unwise to employ them. Third, think of the difficulties in maintaining a close inspection of the fluoride content of milk that is packaged in many different plants in a metropolitan area. These are problems that can be studied and probably solved. In a decade or two, there may be enough knowledge available to permit wide use of one or more other fluoride vehicles. They would be particularly valuable in rural areas.

Along with the discussion of other routes for fluoride ingestion, relative costs usually become a factor. The cost of fluoridation has been estimated to be from 5 to 12 ct per person, depending upon several factors, which include the particular compound used, the number and types of industry also using the water supply, and the size of the community. If the cost is based on the 30.6 percent of our urban population between birth and 19 yr of age, the cost is only 16 to 39 ct per child (*33*). In truth, only a small part of the water in any community is consumed by human beings. But this is a completely irrelevant fact in the determination of whether or not to fluoridate, because the aforementioned cost estimates are based on the total annual cost and not on the amount of water consumed. These estimates are many times less than the annual cost for repair of the ravages of tooth decay in our child population as well as far below the justifiable fees for the less effective topical fluoride treatment.

An appraisal of fluoridation. From the data presented in the discussion of the preceding five questions, we can conclude that (i) our bodies routinely metabolize small amounts of inorganic fluoride that are present in all our foods; (ii) the ingestion of an optimum amount of inorganic fluorides during tooth development results in a 50-percent lower incidence of tooth decay through adolescence and adult life; (iii) the consumption of this amount of fluorides does not result in any toxic manifestations even after long periods; (iv) the fluorides can be as accurately introduced in waterworks as by nature; and (v) there is presently no method of supplying fluoride that is as safe and effective as fluoridation.

In the long and colorful history of public health explorations, it is doubtful that any other public health procedure has been tested with as many patients under as many different controlled circumstances for as long periods. Nature has provided dozens of communities from the north to the south of this country with every conceivable level of water-borne fluorides from the most minute traces up to 8 ppm or more. Thus there has been available for study a wealth of epidemiologic material that could not possibly have been collected in a humanly planned survey. The data from these communities all point toward the rational use of fluorides at a level of about 1 ppm as the only known way to reduce tooth decay in urban populations.

Despite the wealth of information, there are those who are in opposition to the fluoridation of public water supplies. Some oppose this procedure on the ground that fluoridation is a form of mass medication. This is a difficult position to understand, in view of the fact that fluorides are omnipresent in our foods. If the fluorides were entirely foreign to our bodies as they would be if there were no fluorides in our common foods, there might be a little logic in this belief. However, since we consume in the neighborhood of 0.2 to 0.5 mg of fluorides during an ordinary day, fluorides must be considered in some other category than as medicine. In my opinion, the threefold increase in fluoride ingestion resulting from fluoridation should be considered as the fulfillment of a developmental requirement. I believe that it is much more reasonable to compare fluoridation to the enrichment of white flour where calcium, iron, riboflavin, and niacin are added, to the irradiation of milk to increase its vitamin D content, or even to the use of artificial heating in cold climates.

Occasional allusions are made to the possibility that fluoride introduction into the water could serve as a saboteur's tool. These become ridiculous when examined in detail. Since no generalized systemic fluoride toxicity occurred after consumption of water containing 8 ppm of fluorides for prolonged periods, levels well in excess of this concentration would have to be consumed for long periods to cause any chronic toxic manifestation. Obviously any prolonged increase of this magnitude would be too readily detectable to be useful in sabotage.

The only other possibility to use the fluorides as a saboteur's weapon would be in the acute toxicity phase where the safety factor is 1000 to 5000 times the amount readily available from water containing

1 ppm (*21*). If water were to contain about 1000 ppm, the consumption of 8 oz by a 10- to 12-yr-old child would produce severe nausea and occasionally death. This concentration, of course, would be so salty and obnoxious that it is unlikely anyone would swallow more than the first mouthful. A simple calculation for the Metropolitan District Commission Water Supply for Greater Boston indicates how completely impractical this level would be in sabotage. On an average summer day, approximately 210 million gallons of water are used in Greater Boston. To increase the fluoride content of this amount of water to a level of 1 ppm by the use of sodium fluoride, 2.1 tons per day would be needed. If a saboteur were to contemplate sodium fluoride as a weapon to produce acute toxicity, he would be faced with the necessity of introducing at least 1000 times the normal amount or 2100 tons (50 boxcar loads) per day. Chlorine and some other chemicals used in waterworks would prove to be much less laborious for this purpose.

Opponents of fluoridation frequently talk about the toxic manifestations that occur at very high levels of fluoride ingestion as if they likewise occurred when water contains only the recommended amount. No scientist would disagree with the statement that fluorides are poisonous at very high levels of ingestion. What is not pointed out by the opponents is the now well-established fact that there are no toxic manifestations at the recommended levels of fluoride ingestion. Like so many substances that are essential to our well-being, the fluorides have a broad spectrum of physiologic influences, ranging from deficiency signs at suboptimal levels of consumption to a definitely beneficial phase at optimal levels, thence to a toxic phase at a much higher rate of intake. It is of the greatest importance to recognize the difference between these three stages and, particularly, the difference between the second and the third. Where the safety factor between the toxic and the beneficial levels is as large as for fluorides, no reason exists for withholding the benefits of the active agent from the public.

It is understandable and desirable that there should be free discussion of the various phases of the fluoridation proposal at the community level. The freedom to dissent from a commonly held opinion and the right to distribute a dissenting viewpoint through available channels are among the most precious heritages of our democratic life. Certainly there is no scientific proponent of fluoridation who would wittingly dispense with either of these tangible evidences of our representative form of government.

For this reason, among others, it was my personal conviction until 4 yr ago that we should patiently wait for and energetically encourage the accumulation of additional facts, both on the potential beneficial influences and on the possible toxic manifestations of fluoridation in the survey areas. In my opinion, these facts have become available in an incontrovertible fashion during this period. I believe that, beyond the shadow of a doubt, we can conclude that fluoridation is both effective and safe. Many independent investigators in private and state-supported universities and in other private scientific institutions hold this view. Practically none are opposed, and very few are undecided or feel that we should wait for more evidence.

Use and abuse of the freedom of speech in public health matters often cause delays in the beginning of worth-while programs. As long as the issues raised are based on facts and on sound reasoning about these facts, these delays should not be considered to be unmerited, and the motives of the opponents in no way should be condemned. However, in connection with the fluoridation proposal, there has been a deplorable introduction of unsubstantiated statements guised as facts. These misrepresentations have led to anxiety on the part of some of our citizens who realize their own inability to evaluate scientific data and seek reliable guidance on such matters.

As valid and understandable educational materials about fluoridation are made available to our urban citizens, and as they see the readily visible benefits among their relatives and friends in neighboring communities, there will be an irrevocable and increasingly insistent demand for the adoption of this public health measure even in areas where the opposition is presently the most vociferous. Already the fluoridation of community water supplies has been widely instituted throughout the United States and elsewhere in the world. As of 1 May 1954, fluorides were being added to the water supplies in 944 cities and towns in the United States with a total population of nearly 17 million (*34*). Many other communities are in some phase of equipment purchase and installation. Practically every month one or more new communities are added to the list of those that have approved fluoridation. While this article was being written, the Chicago City Council approved fluoridation and ordered their Department of Water and Sewers to have the program in operation by 1 January 1955 (*35*). This water system serves about 3,600,000 residents of the city and an additional 500,000 in the suburbs.

The only foreseeable tangible danger with re-

spect to the fluoridation of public water supplies involves the possibility that its dramatic benefits may lessen the willingness of the public to support dental research or decrease the zeal of independent investigators to continue their research concerning tooth decay. Even when all urban areas with water systems have incorporated optimum amounts of fluorides sufficiently long to attain full effectiveness, tooth decay will not be fully prevented among city-dwellers. In addition, the inhabitants of rural areas, which comprise approximately 45 percent of our population, will still be untouched. There can be no longer any doubt that the fluoridation of public water supplies represents the first major step in the prevention of tooth decay, yet it is not an end or complete answer in itself. We must increase the search for other ways to combat tooth decay and the other dental diseases. None of these potential methods are likely to replace fluoridation, but rather they may be expected to supplement its action or increase its effectiveness.

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What we need is not the will to believe, but the wish to find out, which is the exact opposite.—BERTRAND RUSSELL.

The World's Principal Food Plants*

KARL S. QUISENBERRY

Dr. Quisenberry is assistant director of Crops Research, Agricultural Research Service, U.S. Department of Agriculture. In this capacity, he is concerned with the USDA cooperative research on crop improvement at nearly 200 locations in the United States, Canada, and several Latin American countries. Before assignment to his present position, Dr. Quisenberry served as assistant chief of the former Bureau of Plant Industry, Soil, and Agricultural Engineering, and as head of the Bureau's Division of Cereal Crops and Diseases, in which he directed research on the production, improvement, and disease control of barley, corn, grain sorghums, seed flax, oats, rye, and wheat. From 1936 to 1946 he was stationed at Lincoln, Nebraska, in charge of the USDA regional cooperative breeding program for hard red winter wheat, oats, rye, and barley. Before joining USDA in 1925, he was instructor in agronomy and assistant agronomist at the University of West Virginia and the West Virginia Agrcultural Experiment Station. Dr. Quisenberry, a native of Denison, Texas, was trained in agronomy at Kansas State College and in plant genetics and pathology at the University of Minnesota.

THE foremost question in agricultural research today is posed by the hungry peoples of the world. How can we obtain more food? Science has made remarkable strides in crop improvement during the past 50 years. In America record-breaking harvests of the important food crops have become commonplace. But there are hundreds of millions of people in the world who never have enough to eat.

Hunger is on the increase. Advances in food production are barely keeping pace with increases in population. A recent world food survey shows that during the past 20 years the average per capita food supplies of most of the Far East have declined 10 percent. Similar reductions have occurred in North Africa and the Near East.

One difficulty, of course, is that improved techniques are not generally being used in the areas where the need for food is greatest. If improved crop practices were in wide use and the harvests were well distributed around the world, we could go far in alleviating today's hunger. But many national economies are not yet organized or equipped to make fruitful use of scientific agriculture. This problem is being attacked on many fronts—by the governments of the countries where hunger is widespread, by the Food and Agricultural Organization of the United Nations, by cooperative efforts such as the Colombo plan of the British Commonwealth and the technical assistance programs of our own country, and by private foundations and other agencies.

In this symposium we are approaching the world food problem from the viewpoint of fundamental research. Here is how Paul Mangelsdorf described it when he suggested the subject:

> A dozen species of plants and a smaller number of species of animals literally stand between mankind and starvation. We ought to know as much about each of these species as our engineers know about our bombers. Today we do not.

Mangelsdorf proposed that we examine these species as biological entities; that we ask what makes the biological system effective in each instance; and that we see, if possible, where opportunities for improving the biological system may lie.

Among the plant species this would involve consideration of rice, wheat, maize, potatoes, sweetpotatoes, common beans and soybeans, the sugar plants—cane and beets—cassava, bananas, and coconuts. In one part of the world or another, these are plants that provide most of the fuel for man's energy.

The cultivated species have been associated with man from ancient times. Their beginnings are shrouded in obscurity. We do not know when they were first domesticated or at what stage they became dependent on man's care.

In his long association with most of these species, man has altered them profoundly. We cannot trace

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A field of young rice in Indonesia. 1952

their lineage. We know that variations occurred and became stabilized in civilizations that have disappeared.

It is difficult to explain why man, knowingly or otherwise, chose these plants for food. In some cases it was pure circumstance. They have certain characteristics in common. All of them produce an abundance of materials in food that can be easily assimilated by man. All are relatively easy to harvest. The food part of the plant or its primary derivative can be easily stored for long periods of time and can be carried over long distances. Another common quality is their ability to grow under varying climatic conditions. This has made it possible for man to carry some of them with him in his migrations around the globe.

Harvesting of rice in Indonesia is done entirely by the women. 1952

During the past half-century, as we have developed a better understanding of genetics and related plant sciences, we have been able to make further modifications in most of these species. We have shaped them to meet our need for plants that can endure extremes of temperatures, that carry resistance to a multitude of crop pests, that can make efficient use of moisture and fertilizers, and that are suited to machine handling.

Today, man cultivates food plant species of great commercial importance on millions of acres. Often they are grown to the exclusion of other plants, in vast pure stands of genetic materials. This practice makes a crop particularly vulnerable to attacks by weeds, insects, and virulent plant diseases. The rise of a form to which a widely grown variety or related varieties are susceptible means that plantings over vast areas may be seriously injured or wiped out. Since new forms of crop pests are continually being developed in nature, plant scientists must make a continuing search for sources of resistance to be incorporated in new varieties.

Rice heads the list of the important plant species that supply food for mankind. Half of the world's people—70 percent of those in the Orient—depend upon this cereal, wholly or in part, for their sustenance.

Rice requires warm temperatures and abundant moisture for growth. It is cultivated on all the continents and Oceania. But nine-tenths of the world's rice crop is grown in the Far East. There it is produced by traditional methods at a high cost in human energy and soil fertility. Moreover, the continuing political unrest and social upheaval in the rice bowl have kept the Asiatic farmer from increasing his efficiency. Yields per acre have declined. To meet the enormous demand for more rice, more land has been planted to the crop. This has helped to boost world production by more than a million metric tons over prewar levels. Higher yielding varieties, fertilization, and labor-saving methods in the United States and gains in efficiency in other rice-producing countries have also contributed to the increased production.

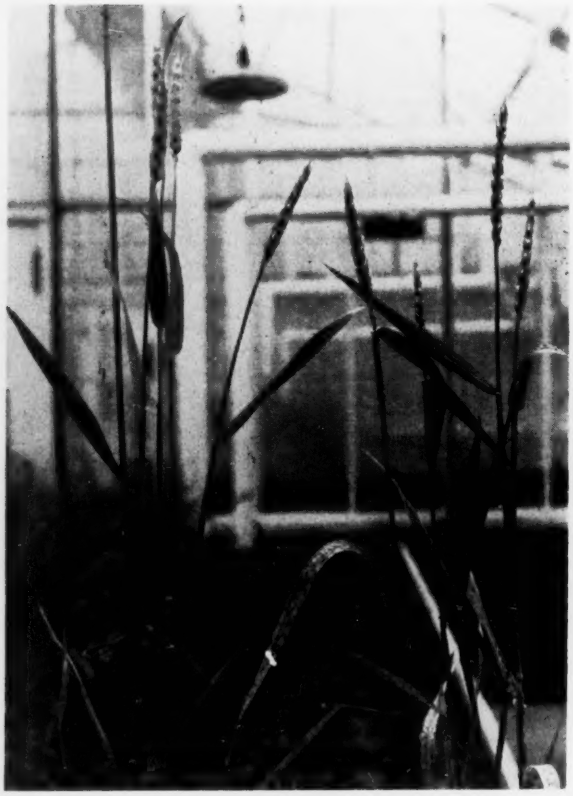
Research in rice improvement, particularly in the Far East, holds great promise. H. H. Love, formerly of Cornell University, reports that nine selections under test in Thailand gave yields from 30 to 78 percent above standard varieties used as checks. The test material came from individual head or plant selections. These were made by Love and his associates as a rapid means of getting better yielding varieties. Possibly greater gains may be obtained from the crossing of specially selected parental material.

Rice produced at the cooperative agricultural station, San Andres, El Salvador. 1949

A wooden rice mill is used on many small Cuban farms for milling the family's grain. 1944

Wheat, the second of the great cereals used by man, is cultivated in all parts of the world where climates are cool and rainfall is moderate. Exclusive of the Soviet Union, for which we have no estimates, world wheat plantings have been expanded by 7.4 million acres since 1938. Larger plantings and higher yields per acre have boosted world production by 16 million metric tons a year. Since most of the leading wheat-producing countries harvest more wheat than they can sell through normal trade channels, the export marketing is handled under an International Wheat Agreement.

Wheat production in the Western Hemisphere is now threatened by one of the most destructive diseases on record—stem rust Race 15B. It caused heavy losses in the spring wheat belt of North America in 1950 and again in 1953. With weather favorable to the development of the rust, Race 15B could destroy tremendous plantings. None of the present commercial varieties of this hemisphere



Strains of wheat that have been put through the screening test at Plant Industry Station, Beltsville, Md., for resistance to rust Race 15B. Left, a strain with high resistance; right, a highly susceptible strain that has become badly infected.

carry resistance to the disease. The United States has joined with Canada and Latin America in an intensive search for sources of resistance. We have screened more than 13,000 wheats in the world collection and are testing the promising material under a wide range of climates and soils.

A chart of world maize production, the third great cereal that feeds mankind, is of interest in that it reflects increases resulting entirely from higher yields per acre. Since 1938 the area planted to maize has been reduced by about 2½ million acres. Yet annual production has risen by 23 million metric tons. We can credit this remarkable gain to the use of hybrid maize.

Another important food plant in which yields per acre have been notably increased over the past two decades is a tuber—the potato. This species, like maize, had its origin in the Western Hemisphere. Today 80 percent of the world crop (exclusive of the Soviet Union) is produced in Europe.

The greatest gains in potato yields have been made in the United States. These have come through disease-resistant varieties, more efficient disease and insect control, certified seed, and improved fertilizer and cultural practices. They have helped to boost yields in the United States from 116 bushels per acre—the average from 1934 to 1938—to 244 bushels per acre, from 1949 to 1951. The crop is under the continual threat of several diseases. Any one of them could eliminate the potato as a leading food crop of North America.

Hybrid corn grown under irrigation and controlled fertilizer application in the Columbia River Basin. 1947

Potato field in blossom near Presque Isle, Maine.

A third crop of New World origin is the sweetpotato. This starchy root is one of the richest plant foods and is high not only in carbohydrates but in vitamins and minerals. It has been highly prized in the Orient since late in the 16th century, when it was introduced in China to relieve famine.

Nearly half of the 10 million metric tons of sweetpotatoes produced in the world each year are grown in Japan. Much of the world crop does not enter commerce. In many tropical islands sweetpotatoes are grown in every garden patch and constitute an important part of the family food. There is probably some good reason why few sweetpotatoes are grown in Italy or India. One could expect that these starch roots would be highly valuable in these and other nations where the climate is hot and moist.

For many people in the world, the next plant on our list is the leading, sometimes the only, source of protein. I refer to beans—specifically the large-seeded *Phaseolus* of New World origin. This warm seasonal annual is cultivated around the globe, and the largest plantings are in China and Brazil. The highest yields per acre are produced in the United States. These average 40 percent above the yields of

Wild potatoes collected in Mexico. The fruit, which carries the true seed of the potato, forms only when the plant has long cool days.

Shucking beans in Haiti.

other high-producing countries and reflect the influence of improved varieties with increased disease resistance and the use of effective chemicals for pest control and fertilization.

Twenty-five years ago a discussion of the principal plants that feed mankind would have dealt with soybeans almost entirely from the standpoint of the crop's production and use in Asia. It has been cultivated in the Far East since ancient times. It is a highly valued source of both oil and protein. But 25 years ago it was not grown to any great extent in other parts of the world. In 1929 there were less than 3 million acres planted to soybeans in the United States. There has been a phenomenal growth of the soybean industry here since that time. By 1950 these plantings had expanded to about 15 million acres, and the soybean harvest of the United States accounts for well over a third of the world production.

Through intensive research, particularly in the development of improved varieties adapted to the soils, climate, and day-length cycles of the central part of the United States, soybean yields have been doubled. New varieties have been developed with higher oil content and with greater suitability to machine production.

We include the sugar plants—the sugar cane of the subtropics and the sugar beet of the Temperate zone—among the important species that feed mankind. Although sugar is still a luxury in parts of the Orient, it is considered a low-cost energy food throughout the Western World.

Perhaps more research has been devoted to sugar plants than to any other commercial crop. The cane industry in almost every country has been saved from ruin by destructive diseases through the introduction of breeding material that is resistant or tolerant to serious hazards. The beet industry had its beginnings less than 200 years ago in the laboratory of the German chemist, Marggraf, and the plant-breeding work of his student, Achard. Continuous and intensive research has enabled the industry to flourish in Europe and to become well established in North America, the Near East, and Japan. Today, beets provide nearly a third of the world's sugar supply.

The next plant on our list is a starchy root that is the staff of life for millions of people in the tropics—cassava. It is known in our part of the world as the source of tapioca starch. Cassava roots can be cultivated with a minimum of labor. They produce high yields—more than 10 tons per acre—in hot, seasonally dry climates where neither potatoes nor corn do well. The starch content of the roots varies from 15 to 30 percent and is higher than that of potatoes. We have no estimate of world cassava production. Nearly all the crop is consumed in the regions where it is grown.

The banana is another staple food in the tropics, where it is most extensively used when the fruit is

Roots of a cassava plant of Thailand. Tapioca, in the form of flour or granulated tapioca produced from cassava, is one of the principal farm products of the southeastern region of the country.

A banana grove in Honduras.

green. At this stage the starch content is high and the bananas can be cooked as vegetables or dried and ground into flour. Although bananas originated in the Far East, they are grown most extensively today in the American tropics. About 75 percent of the world banana crop is produced in Brazil, Mexico, Honduras, and Jamaica. The large, coarse herb, perhaps the largest of the herbs, depends almost entirely on man for propagation.

Finally, we have included the coconut among the principal plants that feed mankind. The palms that produce this useful food grow on the shores of all tropical lands. The fruit provides the primary source of food for many millions of island people. The coconut palm differs from the other principal food plants in that over much of the world it is a wild, rather than, a cultivated plant. Even the large planted groves in the Far East are not tended in the way that other food crops are cultivated.

Man has been able—in his long and intimate association with these plant species—to use them to his advantage. He has manipulated changes in them without understanding fully the structure or the processes of the plant.

What does he need to know to continue making effective use of these plant species? What are their potentials? These are questions that should be answered for each species.

The art of instructing and enlightening men . . . [is] the noblest portion and gift within human reach.—D'ALEMBERT.

Radio and Television*

C. V. NEWSOM

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RADIO broadcasting on a commercial basis was inaugurated in the United States in the early 1920's. Only two decades later the science of electronics had extended the principles underlying radio to make possible the satisfactory transmission of image along with sound; thus for the advertiser, the educator, the sociologist, and many others, a new historical period, the age of television, had commenced. Radio is still and will remain an important medium of communication, but in a modern symposium on the transmission of ideas it is evident that greater attention must be given to television than to radio because of the greater versatility of the former medium.

The first standards for television broadcasting were announced by the Federal Communications Commission in 1941. However, owing to the interference of World War II, the commercial production of television receivers did not begin until 1946. The production of receivers in this country during that first year was only 6500. By 1948, the production figure had climbed to nearly 1 million, and by 1950 the figure was approximately 7,500,000. In November 1953, it was estimated that nearly 27,500,000 television receivers were in use in this country, and about half of all the families owned at least one set. It is significant to note that, among the various occupational groups, the greatest percentage increase in ownership of television receivers during the past year took place among families headed by unskilled workers and farmers. It is claimed authoritatively that the greatest occurrence of ownership in the United States at the present time is among families headed by craftsmen and skilled laborers. The Market Research Corporation of America has also reported that in July 1953, 53 percent of America's families with six or more members had television receivers; 58 percent of the families with four or five members possessed receivers; but that only 38 percent of the families with one or two members owned television sets.

In 1946, nine television transmitters were operating in this country on an essentially experimental basis. By 30 September 1948, the number had increased to 109; on that date the Federal Communications Commission instituted a "freeze" on authorization of new stations pending a review of the original system of assigning television channels. This "freeze" continued until 14 April 1952, when the Commission announced a new program for the assignment of channels and gave 1 July 1952 as the date on which it would again process applications for new stations. As a part of the new program, 70 ultra-high-frequency (UHF) channels, between 470 and 880 megacycles per second, were added to the previously authorized 12 channels in the very-high-frequency (VHF) range, between 54 and 216 megacycles per second; this expansion permitted the assignment of channels to specified locations in the country in such a manner that virtually complete national television coverage would be possible.

A very significant part of the new plan announced in April 1952 was the designation of channels in 242 communities for "noncommercial" purposes; 80 of these communities were awarded channels in the VHF range, and 162 received channels in the UHF range. It follows as a result of this action that 12 percent of all television broadcasting stations in this country would be noncommercial. According to the rules of the Federal Communications Commission:

> . . . noncommercial educational broadcast stations will be licensed only to nonprofit educational organizations upon a showing that the proposed stations will be used primarily to serve the educational needs of the community; for the advancement of educational programs; and to furnish a nonprofit and noncommercial television broadcast service.

The creation of a category of noncommercial broadcasting stations was not intended to relieve commercial stations of their responsibility to serve the public, for, in the words of the Commission:

> . . . this provision does not relieve commercial licensees from their duty to carry programs which fulfill educational needs and serve the educational interest in the community in which they operate. This obligation applies with equal force to all commerical licenses, whether or not a noncommercial educational channel has been reserved in their community, and similarly will obtain in communities where noncommercial educational stations will be in operation.

On 9 November 1953, there were 308 television broadcasting stations on the air in the United States; two of these, located in Los Angeles and in Houston, were noncommercial. The National Citizens Committee for Educational Television has estimated that 25 additional noncommercial stations will be on the air by the end of 1954; the increase in commercial stations will be several times this number. It is apparent that so-called "national penetration" by television is virtually at hand.

It is generally accepted by students of the subject that television provides a new communication medium that is destined to have profound influence on the thought and attitudes of the American public; already its effects are of greater significance than many suspect. Certainly television is reaching and will reach many segments of the American public that are virtually untouched by other mediums of mass communication; its utilization by political campaigners and by advertisers, frequently involving tremendous budgets, attests to belief in its propaganda value. It is only natural that there should be widespread concern, especially among educational and civic leaders, with policies followed by broadcasters in the development of programs.

Severe criticism of many programs is appropriate from the point of view of the audience; many programs have little merit either as entertainment or as intellectual fare. In justice to the broadcasters, it must be stated that it is exceedingly difficult to develop a sufficient number of good programs, or even ideas for good programs, to fill the heavy broadcasting schedule that is being followed; the pressure on broadcasters has virtually made necessary the telecasting of old films and the rehashing of vaudeville skits of a previous generation. It seems inevitable that those responsible for programs will be forced to utilize our cultural and intellectual resources to maintain a range of programs of sufficient interest to attract a sizable audience. It is important to note also that most of television's programing experts have been transplanted from other mediums such as radio, the theater, and Hollywood; adaptation to television is very difficult; and it is probable that the potentialities of television will not be realized until more persons specifically trained to create and direct programs are available for television.

Fenton B. Turck (*1*) has shown that during the past decade there has been a dramatic rise in mass interest in this country in education and in culture generally. He terms this development "The American Explosion," and states, "The culture and economy of the United States have, in less than a generation, exploded into an entirely new dimension." Additional evidence for Turck's thesis may be seen in the growing conviction and virtual demand on the part of American citizens that television be utilized to a greater extent for the transmission of educational and cultural programs; it is obvious to even the casual observer that this expression on the part of the public is being translated into action by commercial broadcasters; and, of course, noncommercial stations will be devoted exclusively to the broadcast of educational, cultural, and instructional programs. Television is already making available on a limited scale educational, cultural, and informational opportunities that previously have been quite inaccessible to many people. The great variety of programs—in music, in art, in the analysis of current events, and in virtually all the traditional academic disciplines—attests to the adaptability of television. The inherent appetite of man for knowledge and culture can now be satisfied outside of working hours and at home. The acceptance of the new challenge by educators is typified by the following statement of policy promulgated by a spokesman for educational television in North Carolina:

> In its simplest terms, it adds up to a determination to reverse the tradition of a university as a cloistered seat of learning by projecting its total facilities and accumulated knowledge to all the people of the state.

It is important also to note that television, with its multiple separate outlets from a single originating source, can mean a greater utilization of brilliant individuals, special talent, and complicated or expensive properties. The leading authority in any field may discuss his subject once with perhaps more effect than in dozens of different appearances before small groups. One concert of an orchestra or a soloist, one performance of a play, may reach many thousands of persons. Programs telecast from

the station at Iowa State College have shown that an agricultural expert can reach more farmers in a 30-min demonstration than he could in weeks of travel around the countryside.

Although educators, advertisers, and others accept as a premise that television can be utilized effectively for the transmission of ideas, it is still essential that greater confirmation be sought before the hopes of many of us possess complete validity. One of the earliest experiments designed to provide partial confirmation was created in 1948 and was conducted for a period of months by the Special Devices Center of the Office of Naval Research. By means of pretests and posttests, comparisons were made of the learning of equivalent groups of merchant marine cadets and of naval and army reservists when they were taught by typical classroom procedures and by television. Some of the more important conclusions of the experiment have been summarized as follows by Robert T. Rock of Fordham University:

1) All groups of trainees showed important gains in knowledge after participation in even a single 1-hour lesson presented by television. Not only were the television sessions as effective as traditional classroom instruction, but they were usually considerably more effective.

2) Trainees remembered what they learned during the television lessons. Repeated tests showed that more than 80 percent of the knowledge acquired was retained for an interval of 1 month.

3) Most of the men participating in the television training projects liked the television lessons better than either typical classroom instruction or training films. Nearly three-fourths of the reservists preferred television training to the usual classroom procedures, and more than 60 percent of them asserted that the television sessions were more instructive than the average training films they had viewed in the preceding 2 years.

4) Available data indicate that some types of instructional presentations are very much more effective in promoting learning than other types. For example, it was found that dramatic presentations when unsupported by narrative or expository sequences were singularly ineffective in promoting learning. Some dramatic sequences actually confused trainees so seriously that they showed a significantly lower percentage of correct responses after such "instruction" than they showed before the lesson.

The general plan followed by the Special Devices Center in providing instruction to men in widely separated locations has received a recent interpretation in the development of closed-circuit television chains for the indoctrination of sales groups and other assemblies of business men. A recent project sponsored by the Atlantic Refining Company provides an illustration of the plan. According to reports in the press, the advertising manager of the company introduced a new motor oil in a 1-hour telecast from Philadelphia to six other regional meetings from Providence, Rhode Island, to Jacksonville, Florida. Less than a month elapsed between the day he first contracted for the program and the time of the broadcast. An average of 150 persons saw the show in each of the cities and learned for the first time about the new oil and how it was to be promoted. In the past, the procedure would have been to run seven separate meetings. A company official is reported as saying,

> . . . without question the result of the closed-circuit show was the most effective production presentation ever seen by our personnel. As a meeting medium we'd use it every time if we could, but frequent re-use might kill the novelty value.

Additional evidence of a great variety of types may be reported to demonstrate the effectiveness of television as an instrument for the transmission of ideas. For instance, students taking telecourses for credit at Western Reserve University come to the campus only to take the final examination. Final examination grades for such students in one of the courses where comparisons were made averaged 13 points higher than the grades made by campus students covering the same material. Audience interest has been revealed by the fact that teachers receive large quantities of "fan mail"; one instructor who posed two special problems received 1371 replies from the first presentation, and more than 1500 from the second.

A television series called "Let's Read Better," was presented during the summer of 1953 by the Arizona State College over station KTYL-TV in Mesa, Arizona; the course lasted for 13 weeks. Half-hour weekly programs featured such devices as a reading accelerator that moves a shutter down a page of type, forcing the viewer to keep ahead of it, and a projector with timing shutter that enables an instructor to project phrases and numbers for split-second intervals in an effort to increase the viewer's comprehension span. Many viewers reported an increase both in reading speed and in comprehension; others reported an increased interest in reading. As an illustration of still another type of broadcast, Western Reserve University reports satisfactory accomplishments from its telecourse, "Your Child Learns to Speak," which is concerned with speech impediments in children. There is considerable interest at the present time in an elaborate study under development by the Educational Testing Service, Princeton, New Jersey, and the Psychology Department of the University of Houston that has been designed to answer the

question, "How effectively can home nursing be taught by television?" The study, it appears, is to be concerned not only with an evaluation of results obtained through instruction by television but also with the complementary problem of the values associated with differing expository techniques that may be employed before the television cameras.

To continue the development of my subject in a completely systematic manner might very well involve an analysis of the lecture system of teaching, for in the case of some educational television programs there is a close analogy between the method employed by the broadcaster and that used by the skilled lecturer before a large class in a university. Although many educators advocate small classes for most effective teaching, evidence to justify this appears to be lacking. In fact, one thoughtful student of educational methods said recently,

> Perhaps it is better to encourage discussion among students after they have listened to a lecture by a challenging teacher than it is to have discussion within the classroom between students and teacher.

In Cleveland some 200 groups of women assemble several days a week for their morning coffee and to listen to and discuss afterward telecasts on psychology from Western Reserve University; perhaps this informal device makes learning by television more satisfactory than actual classroom experience.

One might approach the subject of this paper as would the philosopher by examining critically the phrase "transmission of ideas." Bertrand Russell has stated his belief that ideas are found in prelinguistic experience. Moreover, he asserts,

> Language immensely increases the number and complexity of possible beliefs and ideas, but is not, I am convinced, necessary for the simplest beliefs and ideas.

Russell's considerations make clear the importance of attitudes, action, and setting in the development and clarification of ideas. Such notions appear to have some significance in the design of broadcasts and provide a partial basis for a study of their effects especially in the case of television.

There is widespread belief that major reliance in the future must be placed upon sociologists and social psychologists for fruitful studies on the effects of mass communications. The work of Schramm, Lazarsfeld, Cantril, Klapper, and others in the field of radio, for example, is well known. The patience that is required in a study of effects has been described very well by Wilbur Schramm in the following words (*2*).

> The present trend of thinking about the study of communication effects is to recognize the full complexity of the problem, and also to recognize that it cannot be solved by any simple and direct attack, but only by analyzing the whole situation minutely and painstakingly, bringing to bear on it all potential evidence from the different social sciences, and then whittling away at the unknown area by means of carefully controlled experiments.

The complex of variables involved in any thorough study of the transmission of ideas, as well as some of the conclusions that seem to be possible, have been stated by Joseph T. Klapper in *The Effects of Mass Media* (*3*). For instance, he concludes that opinions are more likely to be changed when mass communications utilize and slightly redirect existing drives instead of trying to create new ones; also, he finds much stronger reliance by an audience upon persons who are regarded as authorities than upon those of little prestige. With highly educated persons, opinion change on a controversial subject is more likely to occur if complete fairness to both sides has been displayed. Of special interest is the fact that communication to the ear results in more retention of simple materials than does communication to the eye, but the combined use of communication to both the ear and the eye results in more retention of simple material than the use of either method alone.

Much of my discussion, as I indicated at the start, is concerned more with television than with radio, for I am looking at the future rather than the past. As Siepmann (*4*) has written,

> Television's major casualty appears to be radio listening. When a television set is bought, the initial, and to some extent the continuing, effect appears to be a falling off of radio listening, amounting to anything from 40 to 50 percent.

But as he goes on to report, the average person in this country still spends more time with radio than with any other medium of mass communication; of course, this is due in part to the statistical effect of considering the large number of individuals who do not yet own television receivers. Interestingly enough, according to Siepmann,

> There appears, however, to be developing a shift in the locale of radio listening with which the arrival of television may have something to do. Today over half of all radio listening in the U.S. is done outside the living room. . . . In radio-TV homes, even between 6 P.M. and midnight, 37 percent of

all listening occurs in the kitchen, and only 29 percent in the living room. Considerable amounts take place in bedrooms, dining rooms and other places.

It is apparent to most students of the subject that radio and television are destined to become supplementary mediums. Perhaps radio will even gain in popularity for the broadcasting of music and of information, where the auditory response is fundamental, and it will continue to be used by the public in situations where visual attention to television is impossible. Also Seymour Siegel, former president of the National Association of Educational Broadcasters, has made the pertinent observation,

> Whatever its faults, commercial television has now compelled commercial radio, by the creation of a competitive situation, to lift its sights. Herein lies a great hope for tomorrow.

As evidence of the new and significant role that radio is to play, there has been the recent development by a national network of a series of radio programs presenting Shakespeare and lectures by such personalities as Toynbee and Oppenheimer.

As significant mediums for the transmission of ideas, radio and television undoubtedly possess tremendous potentialities. In combination with other instruments of communication, and when properly utilized, they can make possible a state in which culture and knowledge are the common possession of all. Technologic advance has presented man with still another challenge.

References and Notes

* Presented as part of the symposium "Transmission of ideas," sponsored by the American Book Publishers Council, American Textbook Publishers Institute, and AAAS Section M—Engineering, Boston, Mass., 27 Dec. 1953.

1. F. B. Turck, *Sci. Monthly,* **75**, 187 (1952).
2. W. Schramm, "The effects of mass communications, a review," *Journalism Quarterly* (Dec. 1949).
3. J. T. Klapper, *The Effects of Mass Media,* mimeogr. (Columbia Univ., New York, 1949).
4. C. A. Siepmann, *Television and Education in the United States* (UNESCO, Paris, 1952), p. 35.

Ernst Mach

In connection with our series of articles dealing with various aspects of the general topic *Validation of Scientific Theories,* it is appropriate that the front cover of this issue be devoted to a portrait of Ernst Mach (1838–1916), once professor of physics in the University of Prague and, at the time this portrait was made, professor of the history and theory of inductive science in the University of Vienna.

Mach's influence upon modern physics manifested itself at two important points. First, there was his criticism of Newton's laws of motion and the suggestion of a new mechanics in which centrifugal and inertial forces depend only upon the relative accelerations of bodies; this suggestion became the basis of Einstein's theory of gravitation and general relativity (about 1911). Second, there was Mach's suggestion that the physical events inside the atom should no longer be described by the motions—positions and velocities—of the subatomic particles but by the radiation that is emitted by the particles as described by *matrices,* not by coordinates; and this was the origin of the first formulation of quantum mechanics advanced by Heisenberg (1926). Both hypotheses (of Einstein and of Heisenberg) have their origin in Mach's requirement that a physical theory be considered relevant only if observable effects (relative accelerations and radiation) follow from this theory. Description of "real physical phenomena" such as absolute acceleration of astral bodies and local motions of subatomic particles are irrelevant, because no observable facts follow from them. According to Mach, a physical theory is an economical description of observable phenomena, but it is not necessarily a *direct* description. This was in some cases not accepted by Mach's immediate contemporaries but is strongly held by logical positivists.

BOOK REVIEWS

Principles of Biology. W. G. Whaley, O. P. Breland, C. Heimsch, A. Phelps, G. S. Rabideau. Harper, New York. 1954. ix + 694 pp. Illus. $6.

WHEN five professors of biological sciences collaborate to write a textbook of college biology, we have a right to expect some sound scientific writing, and in this book we get it. Indeed, it would take much detailed study to find exceptions to any statements in this splendid work. A sound pattern seems to have emerged from the teaching experiences of the authors.

Universities and colleges are confronted today with a host of high-school graduates of "higher than average" achievement who have come through the upper grades at a time when requirements of the armed forces and technical industries took many outstanding science teachers from the schools. Teaching certificates, often temporary, had to be granted to persons eager but rather ill-prepared to teach science in any expert or critical manner. Rapid turnover of teachers subjected pupils to a disconnected pattern of learning, class size increased, standards of achievement were lowered, emphasis shifted from "scholarship" to "citizenship," and general reading ability deteriorated. These factors correlate closely with abilities to master content in biology courses.

Scientific terminology always presents a tremendous obstacle to beginning students. New technical words must be learned as "hieroglyphic wholes" by the great majority of our students today. The authors of *Principles of Biology* have wisely kept the number of technical terms at a minimum but have included the terms that are necessary to develop an important principle.

A brief and concise introduction is followed by a major section (part II) on the structure and functioning of organisms. The organization of plant bodies and animal bodies is discussed first in a general way, with differences carefully noted. Then follow chapters dealing with cells and tissues, osmosis and permeability, stems, roots, leaves, reproduction in plants, photosynthesis and carbon metabolism, nitrogen assimilation and metabolism, respiration, and the role of oxygen in metabolism. Next come chapters on animal nutrition, circulation, breathing, excretion, body covering and support, movement, nervous system, endocrine system, and reproduction.

Part III treats the principles involved in the development of organisms. Part IV is concerned with the kinds of organisms, their classification, variations in life forms, and life-histories. It represents another major portion of the textbook and includes the usual survey of the plant and animal kingdoms. Part V deals with interrelationships of organisms and their physical environment and with health and disease. Part VI discusses the general principles of heredity, sex determination, human heredity, and evolution.

In colleges that offer only a one-semester course in basic biology, one could select materials from parts I, II, and probably IV and certainly VI in its entirety. When such a course permits lectures, demonstrations, and recitations for five periods a week, all four parts may be covered. The diversity of living forms and the complexity of their organization and development can be only appreciated in one semester.

For those who prefer to emphasize taxonomy and evolution, parts IV–VI should prove most useful. Several hours of field and laboratory work each week are essential to the success of this task. Lee and Breland have prepared a companion volume entitled *Laboratory Studies in Biology*, also published by Harper.

Illustrations for *Principles of Biology* have been carefully chosen, are beautifully reproduced, and are of considerable variety. The style is dignified and coherent, factual and expository. There are no exhibitions of personal biases or philosophies, and teleological implications have been spared. The index is excellent, but there is no glossary. As if one were needed! Summaries, suggestions for further reading, student exercises, sample objective tests, and questions for thought and discussion are lacking. The type face and the format of the book permit comfortable reading.

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Plague. R. Pollitzer. World Health Organization, Geneva; Columbia Univ. Press, New York, 1954. 698 pp. Illus. + plates. $10.

THE "Plague Studies" that have appeared since 1951 at irregular intervals in the *Bulletin of the World Health Organization,* revised and provided with annexes, are now available in book form. The preparation of this manual on plague was proposed by the Expert Committee on Plague at its first session in Geneva in 1949, because the need for an up-to-date reference book was continuously apparent. It was not appreciated when the proposal was accepted that the unfortunate world-rocking revolution in China would result in at least one valuable gift to mankind. Pollitzer, the only living authority on plague and cholera in the Far East, who, through his connections with the Manchurian Plague Preventive Service, the Shanghai Quarantine Station, had intimate contact with these diseases during and immediately after World War II, had to leave his post in China. He was immediately invited to join the World Health Organization at Geneva. Using his linguistic abilities and counting on his vast personal experience, the author of *Plague* has blended reports, papers, and documents written in many different languages, frequently difficult to locate, in a work of high excellence.

Few could have performed the difficult task of synthesizing in an understanding, but critical, manner this bewildering mass of observations and interpretations.

Fortunately a general outline and design was available, since Pollitzer had served in 1936 as the author of the chapter on bacteriology, immunology, pathology, and practical laboratory diagnosis in *Plague: A Manual for Medical and Public Health Workers,* issued by the Chinese National Quarantine Service. Although his name heads only the chapter mentioned, there is irrefutable evidence that he participated in the compilation and preparation of the other sections, for which in his modesty he never asked to be credited. That his early efforts are now amply rewarded by the singular continuity and clarity of presentation, which sole authorship of this quality entails, is gratifying to the reader and must give great satisfaction to the author.

Each chapter of the 10, in which every aspect of plague is discussed, carries the imprint of personal experience, judgment, and wise understanding. Every one is an infallible guide from the past to the present and thus makes thought-provoking revelations, often absent in other recent books of a similar nature. To the teacher with problems of presenting one or another aspect of plague in a concise manner or to the general reader or student who wants information, the book will prove stimulating, because it describes in a vivid thoughtful style the spectacular advances in the treatment and control of this dramatic disease. The expert on microbiology and immunology of plague and the public health official in vector control and epidemiology will not want to miss this opportunity to refresh their knowledge. They will appreciate the painstaking compilation of extensive bibliographies, including several hundred references, which follow each chapter. It appears that every question has been adequately answered; a high standard of accuracy permeates every section and paragraph.

A casual perusal of the first chapter, "History and distribution," and of the ninth chapter, "Epidemiology," should help to alleviate the apprehension created by the dishonest Communist propaganda on the use of the plague bacillus in biological warfare.

Since plague in the United States lingers in the wild-rodent populations of the western states, the observations on sylvatic plague made in other countries and analyzed in "Hosts of infection" (Chapter 6) deserve wide distribution. They should assist those responsible for the education of people who might accidentally become infected with the plague bacillus while hunting in an endemic area. The discussions on the insect vectors (Chapter 7) are amplified by magnificent illustrations of the principal fleas that spread plague and by a special chapter written by F. G. A. M. Smit, the custodian of the invaluable Rothschild Collection of Siphonaptera at the British Museum.

If one compares some of the present-day clinical treatises on plague with the contents of Chapter 8, "Clinical aspects," one promptly becomes aware that intimate personal experience and high clinical acumen guided the author when he wrote this section. During his stay in China for nearly 30 years, he saw hundreds of cases of the different forms of plague, including pneumonic plague in the 1920–21 Manchurian epidemic. The remarkable results of treating plague with sulfa drugs are analyzed, and for the first time in comparative tables the results reported from different parts of the world are brought together. Only a few years ago it would have been considered impossible to reduce the average case fatality rate of 40 to 90 percent to 4.2 percent in bubonic plague. Today even pneumonic plague, which was nearly always fatal, has had to yield to treatment with streptomycin and similar antibiotics.

"Control and prevention," the last chapter, offers the advice of an experienced administrator. The value and the use of various poisons are critically appraised, and it is wisely noted that today in the face of an impending plague outbreak the control of the flea vector is more important than the planless destruction of rats. However, it is repeatedly pointed out that cities, counties, and states in which wild-rodent plague prevails must maintain vigorous suppressive campaigns against commensal rats in order to avoid carriage of the infective agent to the proximity of human habitations.

This comprehensive monograph is beautifully illustrated with microphotographs and color plates. The format is of the high quality usually characteristic of books published in Switzerland. As this book is read and reread, it is appreciated as an invaluable handbook that reviews completely the knowledge that has accrued in the 60 years since the discovery of the plague bacillus.

K. F. MEYER

The George Williams Hooper Foundation for Medical Research, University of California Medical Center, San Francisco

The Magic of Electronics. Edward J. Bukstein. Frederick Ungar Publ., New York, 1954. 256 pp. Illus. $3.95.

A PERSON who has only elementary knowledge of the physical sciences may be acutely aware of the ever-growing multitude of applications of electronic science and yet be at a complete loss as to how to find out about this science without long and perhaps arduous study. In addition he probably has only a vague idea about the meaning of the term *electronics.*

By means of a simple but effective technique, Bukstein explains the elementary principles of operation of representative electronic devices and control systems. In so doing, he has made the scope of the term *electronics* clear. His method is to present a picture or diagram illustrating the subject and then devote a page to discussion. The diagrams are clear and well chosen. Some of the pictures are pertinent, but others might as well have been omitted, for they show little more than the appearance of some types of electronic equipment.

The text is clear and well written throughout. The author evidently has a comprehensive and accurate knowledge of the subject, and he has a knack for explaining a technical subject in an interesting and elementary way without doing so in words of one syllable, as is so often done in popular science writing.

Some of the topics covered are elementary principles,

dielectric heating (including rubber curing, glue setting, and plastic molding), induction heating, x-rays, welding, television, radar, photoelectric devices and applications, electronic control, medical electronics, transistors, and Geiger counters. Naturally, only a small amount of information can be conveyed about each topic.

The book should be of use for collateral reading for physics students in high school and college and for others who have had some basic training or experience in electrical matters. More advanced readers will also enjoy the book and probably will find something new in it because of the wide variety of topics covered.

In my opinion, both author and publisher have done a good job.

R. C. RETHERFORD

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Physics Principles. Stanley S. Ballard, Edgar P. Slack, and Erich Hausmann. Van Nostrand, New York–London, 1954. vi + 743 pp. Illus. \$7.50.

A GLANCE through this book gives this impression: it is a textbook of physics. There is good writing and sound experience backing the claims the authors make in their preface. The authors move immediately into the subject matter without apology and develop the concepts needed as they proceed, without forgetting that the beginning student has a lot to learn. There is no doubt at all that students who are unfamiliar with the concepts of the calculus will be in trouble almost from the beginning, for the text is designed to present physics with the elegance and effectiveness possible only to those who have studied the calculus or are at least studying it concurrently.

In mechanics, mass is represented by m, momentum by mv, and moment of inertia by $I = mr^2$. The authors carefully point out the use of gravitational units in illustrative problems, but they present the logic of physics without the complicating g about which all physics teachers argue so endlessly without ever reaching general agreement. Ample problem material is found at the end of each chapter, with answers given to the odd-numbered problems. This practice finds general approval among students who need the knowledge of some answers to reassure them about their judgment and their arithmetic.

The discussion of harmonic motion along with elasticity permits sound emphasis on highly important physics and allows pendular motion to be exhibited in proper perspective and not as all-important as it was in Newton's time when gravitation was so all-absorbing.

The authors are frank and honest in differentiating between density d and weight density w, which makes it possible to present Bernoulli's principle without confused quibbling.

As in mechanics, so also in heat the authors go about their business directly and with praiseworthy emphasis on the essentials. They are to be congratulated for their careful treatment of the gas laws and their numerical explanation of that constant R, called the "gas constant," which is used carelessly in so many textbooks. The treatment of thermodynamics is traditional, and physics teachers must still await a book that will combine the Clausius statement of the second law with the extremely important topics of refrigeration and the "heat" pump.

In electricity and magnetism, the authors again do a worth-while job with the myriad units and systems of units now plaguing both teacher and student. It is also comforting to find the hydrogen ion still positive and the right-hand rule unencumbered with apologies. Enough of the magnetic circuit is presented to make it useful and to allow subsequent notions on transformers and the like to be presented coherently. This material is surely physics, although the authors yield to temptation in presenting sophisticated topics and gadgets, such as radar. Perhaps the student will be ready for these, however, and feel that he is at last learning about something important.

In combining wave motion with sound and light and placing this material after electricity, the authors have done a very logical and justifiable thing. It is a sequence that will suggest rethinking and rewriting the descriptions of courses in college physics in a good many catalogs, if the teachers of these courses adopt this textbook and decide to play the game according to the authors' design. The diagrams and discussions throughout are clear and instructive, as are also the closing chapters on modern physics.

As remarked in the opening sentence of this review—this is a textbook of physics. It is not a survey, and should not be used by teachers looking for a sop for students wanting to study something easy.

R. F. PATON

Department of Physics, University of Illinois

Introduction to College Physics. Rogers D. Rusk. Appleton-Century-Crofts, New York, 1954. xxii + 816 pp. Illus. \$6.50.

THIS general-interest textbook blends the historical and philosophic, together with some of the implications of discoveries for society, with a sound treatment of fundamental physics. As in some other widely used textbooks at the first-year college level, the subject is introduced with the mechanics (statics) of fluids and gases, an easier but perhaps less logical approach than through kinematics. Illustrative materials are drawn from a wide range of common experience, which includes physiology as well as pure physics and engineering. The treatment of the acoustics of music and the physics of color is full and interesting and seems appropriate for the general student.

The mathematical requirements do not go beyond high-school algebra and trigonometry. Many illustrative problems are worked out, and careful attention is given to units. The mks system parallels the English and cgs systems throughout most of the book. Important laws

and definitions are set off in boldface or italic type. A formal verbal statement is usually followed by the defining equation in words, and the symbolic form of the equation is then given. This device not only should provide emphasis but it should make the student aware of the difference between a formal verbal definition and a defining equation.

Questions and problems are provided at the ends of the chapters, and answers are provided for the odd-numbered problems. An appendix includes a list of fundamental physical constants and conversion factors, and a brief review of fundamental algebra and geometry.

In recent years, a number of excellent textbooks have appeared that present the principles of physics in a clear, logical, and formal manner. The result is sometimes a rather cold, impersonal structure without universal appeal, which is likely to be particularly repelling to the uninitiated. This textbook does not fall in this category. It appears that the author has succeeded well in reaching his objectives without sacrificing a sound treatment of the fundamentals.

W. Paul Gilbert
Department of Physics, Lawrence College

First Year College Physics. Clarence E. Bennett. Ronald Press, New York, 1954. vi + 526 pp. Illus. $6.

THIS new textbook is one of the shorter treatments of physics, and the author states that it is designed for college freshmen. The "chapter-a-week" idea is sound and should save a teacher's time in making assignments. Bennett makes no attempt to be spectacular or sensational, presenting what he considers the essentials in clear statements and using conventional symbolism throughout.

One of the features of this textbook is the long list of problems at the end of each chapter. In general, these problems are of the drill type, which will give the beginner practice in searching out the correct formulas and appropriate units. Answers to the odd-numbered problems are given in an appendix, permitting the learner to try out his ability to use arithmetic with precision.

The chapters on mechanics are traditional Newtonian. Some of the ideas presented are elaborated in too great detail. Projectile motion, for instance, is elaborated far more than its importance justifies. The student will surely feel that this topic must be essential in beginning physics, which is certainly open to question. The tendency to obscure basic physical concepts in excessively detailed geometric and algebraic discussions detracts greatly from the value of the book as a textbook. These labyrinths of geometry are climaxed by a figure showing the relationships between circular and simple harmonic motion. It would be a real task to determine the correctness of such an elaborate drawing, and it is questionable how much essential physics could be gleaned from a careful study of such a geometric maze.

In the main, the author chooses his units wisely, but defining specific heat as a ratio is archaic and leads to errors of statement in subsequent discussion. There is so much important physics merely mentioned, or brushed aside with a formula, in the chapters on heat, that many teachers looking for a new and useful textbook will hesitate to use this one. The use of more than two pages to describe the gas thermometer, for example, as contrasted with less than a page on the gas law, illustrates questionable emphasis in coverage.

Every teacher of physics has his own pet hobbies, and I feel that the chapters on electricity are far better than those on heat. The author weakens his text by apologizing for the decisions he makes on units, current directions, and the like, but a student who has completed this part of the course should be able to proceed effectively in advanced courses. This is also true for the chapters on more modern topics and on light. Some of the diagrams used in physical optics are carelessly done. They show a few unfortunate errors and will leave false impressions in the student's mind. Here again improvements could be made readily enough without enlarging the book.

Since it is easy to find fault, and physics teachers are notoriously dissatisfied with the authors of textbooks of physics, the reader is cautioned to remember that this one has many fine and useful features. It is frankly a drill book, and beginning students certainly need practice. Perhaps freshmen using it would hesitate to pursue physics further. But those who did well in the course would like it and find themselves adequately prepared for advanced work requiring some knowledge of basic physics.

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An Introduction to Bacterial Physiology. Evelyn L. Oginsky and Wayne W. Umbreit. Freeman, San Francisco, 1954. xi + 404 pp. Illus. Trade, $7.25; text, $6.

THE reaction of most persons when they see this textbook for the first time is something akin to the reaction of a traditionalist to a painting by Salvador Dali. But once accustomed to the eye-catching illustrations, readers will find a sound, flowing account of the what, the how, and the why of bacterial function.

It is a good book. It scans well the usual features of bacterial physiology and adds a few new ones, such as the chapter on virulence as a physiological problem. It is well balanced and avoids the current research hypertrophy toward metabolism. However, the undergraduate user for whom it is intended may well find the necessary biochemical aspects, although predigested by the authors, better assimilable if first taken as a separate course. This quandary of basic prerequisites is not new to the teaching of undergraduate microbiology and obviously does not retard the many college courses for which this textbook should be nicely tailored. There may even be a few individuals in the professional stratum who will welcome the opportunity to refresh or update their knowledge in this area with reading and illustrations reduced to their simplest terms.

This new textbook inevitably bears the mark of Wis-

consin's I. L. Baldwin, whose perhaps earliest course in bacterial physiology persists in principle here, and rightly so. Taken, taught, and transcribed in increasingly modified form by Wayne Umbreit, its thread still appears in this book. Both authors, presently absent from active teaching, were able to look back reflectively and ahead in an unbiased way while writing this book—really a very enjoyable way to write a book. The results show it.

Philipp Gerhardt

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A French-English Dictionary for Chemists. Austin M. Patterson. Wiley, New York, and Chapman & Hall, London, ed. 2, 1954. xiv + 476 pp. $6.50.

THIS new edition of the favorite French dictionary for chemists has grown from 35,000 terms to 42,000, and it has been improved in other ways. It could be taken for granted that it covers adequately the technical words encountered in chemical French, and a careful check proves that it does. But its usefulness is not limited only to chemists. I took advantage of a somewhat eclectic library to check this book against works on subjects such as anthropology, vector analysis, and Egyptology. In no case did the dictionary fail to provide an illuminating definition of all words that were really in any doubt, including such words as *dallage, entaille,* and *reboucher.*

The vocabulary, fortunately, is not limited to chemical or even scientific words. It contains a good working list of words in ordinary usage, including common words that may be unfamiliar to those who do not read French literature fluently. As an example, the word *tiers* might be given. Another feature useful to the beginner is the listing of the verb in its various forms when these might appear different to the uninitiate.

The work is really so good that it is difficult to find any fault. However, I did note that among the meanings of *abaque,* the meaning of *nomogram,* which is quite common, was not specifically given.

William C. Boyd

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General Chemistry. A topical introduction. Eugene G. Rochow and M. Kent Wilson. Wiley, New York; Chapman & Hall, London, 1954. xiii + 602 pp. Illus. $6.

THIS interesting and stimulating textbook was written "primarily for the serious student." It deserves the consideration of all teachers who wish to offer an advanced college-level course as a challenge to their most ambitious and capable freshmen. The compact style of presentation of "a unified group of chemical principles . . . to furnish sound fundamental training in chemistry" is illustrated by Chapter 1. In addition to the usual introduction to chemistry and the classification and states of matter, these 19 pages cover the mass spectrograph, systematic inorganic nomenclature, and the use of dimensional analysis to solve chemical problems involving symbols, formulas, and equations.

The usual scattered historical notes have been gathered together in a coherent "History of chemistry" in the appendix, thus providing space for the discussion of such topics as bond angles, quantum numbers, orbitals, piezochemistry, photochemistry and the mechanism of photosynthesis, biochemistry, macromolecules, and a thought-provoking chapter on "Our resources."

Although this book is quite free from ordinary typographical errors, I would prefer to see some items revised: (1) the "free chlorine" theory for the action of *aqua regia* (p. 80); (ii) "deep-red . . . $Fe(SCN)_3$" (p. 158); (iii) the outmoded soda ash process for the absorption of bromine liberated from the sea (pp. 193, 206); (iv) the use of electric *power* for electric *energy* (p. 269); (v) the use of *electrolysis* (p. 270) to describe the electrothermal reduction of silicon dioxide by carbon; (vi) the unusual hypothesis advanced to explain paper chromatography (p. 392).

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Introduction to Chemistry. R. T. Sanderson. Wiley, New York; Chapman & Hall, London, 1954. x + 542 pp. Illus. $5.50.

GENERAL college chemistry is a fairly well-defined field for which many textbooks are available. Therefore, when I receive a new book, I wonder whether it will be a mere "rehashing" of the contents of other books. Sanderson's *Introduction to Chemistry* presents, however, a distinctively new and valuable approach to the teaching and study of general chemistry.

The most valuable feature of this new book, in my opinion, is the decided emphasis on explanatory material rather than on descriptive material. This is definitely a textbook; it makes no attempt to double as a reference book. The organization of subject matter is logical from the teaching standpoint, not from the historical standpoint.

The first several chapters introduce the student to basic concepts of the composition of matter, atoms, molecules, and so forth, and these concepts are used extensively in the remainder of the book. A chapter early in the book ties in the study of chemistry with many common household substances, so that the student will be able to relate chemistry to everyday living throughout the course. The high point of the book is reached in the detailed chapter on periodicity (Chapter 23). This subject is introduced with an appropriate illustration, and subsequent explanatory and descriptive portions are both clear and thorough. The last chapter, on the profession of chemistry, is an interesting "extra," but it may be more abbreviated than necessary.

The author uses a refreshing, informal style. This feature, along with the emphasis on explanatory material, should make the book very teachable. The book is

written to the student, with frequent use of the second person. Among the numerous informal statements obviously intended to arouse interest may be cited two, "The skunk is made of molecules. So are you." "Phenolphthalein serves . . . in . . . commercial laxatives, in spelling contests, and as an indicator."

Many good diagrams contribute to the clarity of explanations. A few are of questionable value (for example, one illustrating activation energy on page 265) other than for arousing interest. Most, however, are of real teaching value (for example, illustrations of factors that influence rate of combination with oxygen on pages 259–263). Study suggestions and questions are included as study helps after some chapters. It is to be regretted that similar helps are not included after other chapters.

The type is large. The author and the publisher are to be congratulated for this excellent teachable book, which may be expected to take its rightful place among the good textbooks of college chemistry.

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Principles of Polymer Chemistry. Paul J. Flory. Cornell Univ. Press, Ithaca, N.Y., 1953. xvi + 672 pp. Illus. $8.50.

WHILE polymer science has been developing into a special branch of chemistry and physics during the last 25 years, a series of comprehensive treatises has appeared, which in most cases either covered certain groups of macromolecular substances, such as proteins, vinyl compounds, cellulose, and the like, or concerned themselves with certain aspects, such as the physical chemistry, the organic chemistry, or mechanical behavior of polymeric materials. These books were essentially descriptive in character and, whenever quantitative formulations were presented, they consisted of more or less specialized theories designed to cover a limited field of phenomena.

Flory's book is of a different character and introduces a new era of literature, presenting our knowledge of polymers in a comprehensive manner. It uses the generally accepted ideas on the mechanism of the formation of macromolecules on their configuration and on their response to external forces, but it uses them systematically throughout the whole presentation in a uniform deductive manner and penetrates to quantitative statements as much as this can be done under present circumstances.

The author is highly qualified to offer such a presentation, because he has personally made just this kind of contribution to all branches of the subject field and can justly be called the leading person in the present development of polymer science.

In addition to this new and modern general approach, the book is remarkable because of its clarity and simplicity and the large amount of actual data that it conveys to the reader. Figures and tables are carefully selected and well reproduced. The references are assembled at the end of each chapter and here it might be permitted to remark that contributions of European authors are perhaps not always quite adequately covered and sufficiently discussed.

Everyone who wants to familiarize himself with the present state of our actual knowledge and with the existing trends in polymer chemistry will draw great advantage from Flory's book, which can be warmly recommended to students, teachers, and industrial chemists as a source of information and stimulation.

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Polymer Research Institute,
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Representative Chordates. A manual of comparative anatomy. Charles K. Weichert. McGraw-Hill, New York–London, 1954. vii + 204 pp. Illus. $3.50.

THIS laboratory manual for comparative anatomy describes in orthodox fashion the organ systems of the lamprey, spiny dogfish, mud puppy, and cat. The verbal descriptions are terse without much theoretical comment. The manual is noteworthy for its profuse use of illustrations. With this manual, a student should be able to master these four animals with little instructional assistance.

J. WENDELL BURGER
Department of Biology, Trinity College

Manual of Child Psychology. Leonard Carmichael, Ed. Wiley, New York, and Chapman & Hall, London, ed. 2, 1954. ix + 1295 pp. Illus. + plates. $12.

WITH the range of its subject matter extending from the babbling of the idiot to the brilliance of the genius, from the movements of the fetus to the ethical behavior of the adolescent, from the anatomy of the embryo to the topology of the life space, the revised edition of the *Manual of Child Psychology*, like the first edition of this remarkably comprehensive book, is an indispensable source of information for the advanced student and specialist. In addition to numerous references to earlier publications, the bibliographies now include 1145 selected references with publication dates between 1946 and 1954.

Fifteen of the 19 chapters were revised by the original authors. Two chapters, "Psychopathology of childhood," by Clemens E. Brenda, and "Social development," by H. H. Anderson and G. L. Anderson, appear for the first time in the revised edition. The chapter on "The adolescent" was revised by John E. Horrocks, and an addendum to the chapter on "Behavior and development as a function of the total situation" was contributed by Sibylle Escalona. Two chapters of the first edition were omitted: "Maturation of behavior" and "The feeble-minded child."

The contributors have undertaken to provide "an accurate and coherent picture of some of the most important aspects of research in the scientific psychology

of human development." If we think of the alterations effected in the several chapters as samples of the amount and importance of serious research since 1946, we find that these samples may readily be divided into three groups. (i) Relatively minor changes were necessary in seven chapters: "The onset and early development of behavior," "Animal infancy," "Physical growth," "The ontogenesis of infant behavior," "Learning in children," "Measurement of mental growth in childhood," and "Behavior and development as a function of the total situation." (ii) Continuing vigorous research and important new developments characterize seven chapters: "Methods of child psychology," "The neonate," "Language development," "The environment and mental development," "Research on primitive children," "Character development in children," and "Gifted children." (iii) Five of the chapters are new or completely rewritten in the light of recent research: "The adolescent," "Emotional development," "Psychological sex differences," "Psychopathology of childhood," and "Social development."

Sixteen of the chapters present systematic reviews of published research. The material for the most part is arranged chronologically under the major topics investigated. Critical evaluations reveal points of view, but the studies reviewed were not selected to illustrate or exemplify only one particular viewpoint.

Three of the chapters present conceptual frameworks or systematic theories that are used to interpret, to integrate, and to evaluate the designs and the results of investigations.

A continuing and ubiquitous problem for developmental psychology is the "integration versus individuation" issue. The new *Manual* reveals an increasing tendency among investigators to select and depend upon either an isolation technique or a field technique to provide the information required for a portrayal of the developmental process. Experimental analysis by isolation deals with limited segments of behavior, and field techniques may be expected to omit information about some variables that may function as critical foci within a total pattern. The limitations of techniques of investigation should not be disregarded whether "integration" or "individuation" appears to be the most useful conceptual model for representing development.

The omission of a chapter on maturation may indicate a trend in child study that was anticipated in the concluding remarks of the chapter on that subject in the first edition of the *Manual*:

> . . . the "maturation-versus-learning" dichotomy is a cumbersome conceptual framework. . . . Once the laws of development have been determined the maturation concept may fade into insignificance.

However, the role of physiological growth has been considered in several chapters, and maturation is the pervasive theme of the chapter on "The ontogenesis of infant behavior." One does find that investigators who give evidence of acquaintance with current research on learning are not disposed to attribute all fundamental developmental changes to a self-directing influence called maturation.

Two other trends should be mentioned. There is an increasing consideration of "dynamic" concepts that have been taken largely from psychoanalytic literature. In some cases, this seems to represent a predilection for the use of broad abstractions, whereas in other cases it seems to represent earnest attempts to deal with practical problems of motivation by means of hypothetical constructs. There are, also, increasing attempts to comprehend the "whole child" without transgressing the boundaries of meaningful communication.

In general, this large book is concisely written. It fulfills its function as a manual exceedingly well. The continuing synthesis or emergence of a more adequate portrayal of child development will be facilitated in many ways by this revised guide to the methods and vast achievements within this field of scientific research.

H. E. Weaver
Department of Psychology, Oberlin College

Methods of Research: Educational, Psychological, Sociological. Carter V. Good and Douglas E. Scates. Appleton-Century-Crofts, New York, 1954. xx + 920 pp. $6.

THIS book, I think, will find widespread use in the scientifically slanted courses in teacher education. Three characteristics are especially noteworthy: the comprehensiveness of the topics covered, the generally simple style of writing, and the attractiveness of the typography.

For some 50 years teacher education has been seeking to introduce students to scientific thinking and to objective methods of research. Education has borrowed freely from psychological research, statistics, and engineering. Although educational research workers often have developed considerable skill in mathematics and statistics, they have had little preservice exposure to systematic treatments of ways to plan, conduct, and report research studies. These skills have been acquired largely by experience on the job.

Awareness of the shortcomings of training programs for educational research workers must have been the factor that spurred the authors into nearly every possible nook and cranny of their subject. This is a good thing for most beginners. They can travel from a somewhat idealistic opening chapter on "research as a way of progress" through chapters dealing with various technical research methods, including such prosaic topics as note-taking. The beginner will do well to stride manfully through this rather long analysis and description; the experienced research worker will dip in and out as his interests and needs require.

The impressive thing is the success that the authors have in covering so much ground while making the textbook so interesting. They have dropped in a few names, anecdotes, and bits of news; they have not hesitated to use the editorial *we*. They have touched upon "social

significance" without flying off on a tangent. They have referred to situations and questions that confront typical research workers in school systems, colleges, and research bureaus. These touches, combined with systematic and comprehensive treatment of research methods, have resulted in an especially useful handbook for all who do research in psychology, sociology, and education.

Most pages carry footnote citations to specific studies and, in some instances, the sprinkling of citations is rather liberal. They are neatly tucked in, however, so that they do not seriously impede the reading. The last four chapters, on the experimental method, case studies, developmental studies, and how to report and implement research, are bolstered with carefully selected bibliographies, in addition to a generous number of footnotes.

Good and Scates collaborated with A. S. Barr in 1936 (revisions in 1938 and 1941) in writing a book on the methodology of educational research. *Methods of Research,* although similar in arrangement and topics to the earlier volume, has been so extensively revised and improved that it constitutes a new book.

FRANK W. HUBBARD
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Introductory College Mathematics. Adele Leonhardy. Wiley, New York; Chapman & Hall, London, 1954. ix + 459 pp. Illus. $4.90.

Introductory College Mathematics. Chester George Jaeger and Harold Maile Bacon. Harper, New York, 1954. xii + 382 pp. Illus. $4.75.

THESE two textbooks, bearing the same title, are both designed to meet the needs of students whose study of mathematics will be confined to a 1-year course. Both seek to introduce material more intellectually stimulating than college algebra and trigonometry and to provide a background in mathematics for those students whose interest may be so aroused by this course that they will continue their study of mathematics. The approach in the two books is somewhat different.

Adele Leonhardy develops the nature and ideas of mathematics by confining her treatment to a few major topics and concepts. Her selection includes the nature of mathematics, our number system and the algebra of numbers and their logical bases, the practical aspects of measurement and computation, functional relationships and the rate of change of functions, and simple statistical measures and their interpretation. Attractive features are the large number of exercises drawn from many fields of interest, chapter reviews, bibliographies, and interesting historical notes. In my opinion, the author's twofold purpose—to develop for the student the mathematical concepts and techniques needed in a program of general education and to present mathematics itself as one of the areas of general education—has been admirably fulfilled.

The book by Jaeger and Bacon is not pointed so directly toward general education. It follows more nearly the traditional pattern of textbooks in freshman mathematics and should prove a very teachable book. Topics in algebra are not stressed but are interwoven in the other material. The treatment of trigonometry and analytic geometry is adequate for the study of further courses in mathematics. The calculus is introduced rather early, and differentiation and integration of trigonometric, logarithmic, and exponential functions are included.

In the early chapters, the authors supply rather complete details to enable the student to develop confidence in himself. As the book proceeds, more is left for him to supply, in an effort to develop his courage and imagination. The tables in this book are somewhat more extensive than those in Leonhardy's book but are not adequate for accurate computation.

H. S. POLLARD
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Inorganic Quantitative Analysis. Carroll Wardlaw Griffin. Blakiston, New York, ed. 2, 1954. xvi + 417 pp. Illus. $4.75.

MUCH as a saint would meditate upon the mysteries of the Rosary, a good teacher should occasionally meditate upon the "mysteries" of his subject. The chemi-saint will immediately recognize that the magnificent structure of chemistry rests securely upon the foundation of quantitative analytic fact and less securely upon the fruits of the keen imagination of his chemical predecessors. A good textbook of quantitative analysis must make this point crystal clear to the student. After all, the reason we teach "quant." is that it is useful, and certainly we are obliged to see that the student clearly understands the usefulness of this subject.

Therefore, an acceptable textbook of quantitative analysis should emphasize the absolute veracity of experimental fact. It should help the student to acquire a good laboratory technique. It should indicate how the student can determine whether he has, indeed, established a valid and useful fact or has erred somewhere in his procedure. It should warn him that the procedures in the textbook are rather special and will not always work and where to turn in that event. Above all, the student must be made to realize the reason why quantitative analysis is necessary for almost every chemical investigation.

This is a standard elementary textbook. It starts with a word of caution to the student and general operations. There follows a special chapter on separations; seven chapters on volumetric analysis, including a thorough treatment of pH, indicators, and titration curves; three chapters on gravimetric analysis; and a chapter each on precipitation titrations, potentiometric titrations, electroanalysis, and colorimetry. The experiments are standard and follow the theoretical discussions. There is an interesting determination of tetraethyl lead in gasoline. The appendix contains a list of worth-while reference works, a table of logarithms, and directions for the care of platinum ware. The index is good. There are several problems at the end of each chapter.

In any treatment of quantitative analysis, there must be interspersed a constant theme, repeated again and again, stating why the subject is important, emphasizing the validity of the experimental fact, suggesting that in later practice the textbook procedure will not often work and must therefore be modified, and explaining the use of the imagination to build better theories of matter, always based on experimental results. No such constant repetitive theme was found. This textbook is satisfactory in all other important respects.

JAY A. YOUNG

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Rocks and Mineral Deposits. Paul Niggli. Translated by Robert L. Parker. Freeman, San Francisco, 1954. xiii + 559 pp. $12.

PAUL NIGGLI lived to see his *Gesteine und Minerallagerstätten* (*Erster Band*) translated into English, but it is the profession's loss that his death occurred a year before Parker's excellent translation was printed and ready for distribution. Niggli had no illusions about the reception that his book would get, for he wrote in his introduction:

> . . . the science of rocks and mineral deposits is one in which scientific controversies have abounded. These . . . flare up again and again, and never end.

The experts will indeed find much to criticize, but Niggli's critics will have to proceed with caution, for they are dealing with a researcher whose innovations and heresies were not put into print lightly or capriciously. On the contrary, most of them are so provocative that it may be doubted whether, with this volume on the market, textbooks in petrography will ever be the same. The book is not designed for casual reading. It is a combination manual of petrographic procedures and techniques for the graduate student, of original thinking in some fields, and of painstaking synthesis in others.

The volume plunges into geochemistry after a very brief introduction. On page 15 the reader is given "the Niggli values" of important rock-forming minerals, and he has the choice of learning the Niggli method of calculation or of proceeding through the rest of the volume at his own risk. Whichever choice he makes, he will wonder how Niggli values became one of the elements of geochemistry. His appreciation of the chapter on crystal chemistry is likely to be tempered by Niggli variations on the more familiar chemical formulas in standard use for minerals and by the introduction of a simple shorthand or speedwriting method for the "formulation" of the chief minerals. Simple as the method is, it gives even the most attentive student one more memory system to master.

Following 137 pages of chemistry, the author devotes more than 200 pages to the physical characteristics of rocks and minerals. He introduces the student to some very useful applications of statistical theory to the analysis of mineral aggregates before he deals exhaustively with structural and textural features of the fabric. His handling of thermodynamics and related physical-chemical principles governing the formation of minerals and rocks invites comparison with the approach employed by Turner and Verhoogen, who use this subject as a preface and a base for the development of igneous and metamorphic petrology. Niggli's late introduction of thermodynamics and physical chemistry results in better integration, but he promptly inundates his reader with a flood of graphic detail, from which the all-important principles are belatedly rescued in the chapter that sets forth the preparatory stages of the crystallization processes.

The book ends with a systematic classification that will undoubtedly be the focal point of the controversies Niggli anticipated. The sedimentary petrologist will see little merit in his superficial handling of the exogenetic rocks, in which he obviously took a minor interest. His new classification of the endogenetic rocks is far more penetrating, although perhaps not as "new" as the author thought. To the student who has applied himself to Niggli's methods, some of which are ingenious and all of which are useful, the book will bring a comprehension of petrography and petrogenesis that is thorough and profound. It may have the Niggli stamp but no more so than petrology according to Iddings or Johannsen. But what great teacher has failed to impart a certain degree of idiomorphism to his students?

HOWARD A. MEYERHOFF

Scientific Manpower Commission, Washington, D.C.

Petrography: An Introduction to the Study of Rocks in Thin Sections. Howel Williams, Francis J. Turner, and Charles M. Gilbert. Freeman, San Francisco, 1954. x + 406 pp. Illus. $6.50.

A WELCOME addition to a new series of geology textbooks is *Petrography: An Introduction to the Study of Rocks in Thin Sections.* The subtitle must be used to indicate its full significance, for therein lies the principal value. Prior to the publication of this book, there existed no modern introductory textbook on the petrography of igneous, metamorphic, and sedimentary rocks, based on the study of such rocks in thin sections. Too frequently, the study of sedimentary rocks has been set apart from the study of igneous and metamorphic rocks. The net result has been the neglect of the study of sedimentary rocks in thin sections and a lack of transition and relation of the three groups to one another.

This textbook is divided into three parts: igneous, metamorphic, and sedimentary rocks. Each of these parts is organized similarly, with introductory chapters on origin, textures, classification, nomenclature, and so forth, followed by petrographic descriptions of a few broad rock clans in subsequent chapters. In fact, an admirable characteristic of this treatment is the attempt to confine classification and nomenclature to a few well-

accepted rock names rather than to confuse the literature with newly coined terms.

The authors have written this textbook on the assumption that the student is already familiar with the megascopic study of rocks in hand specimens and with the principles of optical mineralogy. Numerous excellent photomicrographs of rocks in thin section are of value for portraying the mutual relationships among the constituent minerals as well as the texture and fabric of the rocks concerned.

In the words of the preface:

> This book deals more with the description of rocks than with problems of genesis, more with petrography than with the broader field of petrology.

This approach is most desirable in an introductory course. However, there is sufficient discussion of the petrogenesis of rock types and clans to whet the appetite of the beginning student of petrology. Frequent consideration is given to the possible transitional relationships between igneous and metamorphic rocks as well as criterions for distinguishing between rocks of metaigneous or metasedimentary origin, for example, the amphibolites. The process and problem of granitization are also discussed to a limited extent.

It is regrettable that more space is not given to the argillaceous rocks, which quantitatively constitute the major subdivision of the sedimentary rocks. It is recognized by the authors, however, that there are inherent difficulties encountered in this group that limit their study with the petrographic microscope.

J. Robert Berg
Department of Geology, University of Wichita

Physical Geology. L. Don Leet and Sheldon Judson. Prentice-Hall, New York, 1954. ix + 466 pp. Illus. $6.75.

THE last few years have seen the appearance of a number of excellent new textbooks on physical geology. Each of these may be distinguished from the old standbys by its profuse illustrations, which include large "bleed" halftones and dramatic explanatory diagrams of the various phenomena. Leet and Judson's new book is no exception to this general rule. It is well illustrated with new figures, diagrams, and photographs. Section headings are made to stand out and new terms are emphasized with italics. The organization and emphasis are, of course, slightly different from those of any other textbook and probably do not coincide with anyone's course outline except the authors'.

The introductory chapters on the scope of physical geology and the materials of the earth are more comprehensive than those found in many other textbooks, and they may be just a little heavy on physics and mathematics for an introductory textbook. This criticism might also be applied to the chapter on earthquakes and the earth's interior. However, the authors state in their preface that this course was followed to promote accuracy at the expense of oversimplification.

The chapter on sedimentary rocks is possibly a little too sketchy, and surely fossils deserve more than a short paragraph. Erosional phenomena are divided into the usual components. The erosion cycle concept is briefly discussed, and the objections to this concept are outlined. As a matter of personal prejudice, a more complete presentation of this subject is desirable, although in one recent textbook it was ignored entirely. The paragraph on sediments and related forms is very well presented, although desert erosional forms and the erosion cycle in arid regions are neglected. Since we live in a country of vast deserts, it would seem desirable to give them particular attention, for the benefit of the casual student.

A similar criticism can be made of the chapter dealing with the oceans. Although there is a wealth of unusual material presented on submarine topography and other interesting phenomena, there is little space devoted to tides, waves, and shoreline forms—things that can readily be seen by the casual visitor to the ocean beach or lake shore. Strangely enough, the discussion of igneous activity is widely separated from that of igneous rocks and shares a chapter with metamorphism. Each of these subjects could be treated individually.

The concluding chapters on the earth's age and place in the universe, mineral deposits, and fossil fuels are well developed, as is the complete and lucid glossary. The five appendixes contain a wealth of additional information, although the time scale and the section of topographic maps could be expanded and improved.

All in all, this book represents an adequate approach to the introductory phases of physical geology, with a good coverage of the subjects that the authors considered to be important. Probably no single teacher will ever be satisfied with any textbook that he has not written himself so that criticism is actually a matter of personal prejudice.

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Museum of Geology,
South Dakota School of Mines & Technology

The Juvenile in Delinquent Society. Milton L. Barron. Knopf, New York, 1954. viii + 349 pp. Illus. $6.75.

MUCH has been said in recent years about the responsibility for the obvious increase in juvenile delinquency. It is the thesis of Milton L. Barron that juvenile delinquency is caused by failure to approach human relationships scientifically. This book is presented primarily as a textbook for courses on juvenile delinquency and criminology. It is also a very readable discussion of the subject, for it is concerned with social problems as a whole and with the etiology of social disorganization.

The book is divided into three parts. Each chapter is followed by questions, research suggestions, and a

list of selected readings and references. Part I defines delinquency and considers its dynamics including material from anthropology, psychology, and psychiatry. Part II is an analysis of the etiological factors. Part III takes up the disposition and treatment of cases.

It is of interest that Barron does not consider juvenile delinquency as a new problem, but as something that has extended back through the ages. Besides referring to the literature, he amplifies his discussion with a series of illustrations by an 18th century artist. This is not a happy story, but it points out, in the concluding paragraph, that

> It is the duty of the social scientist to add that American society too is an indivisible whole. It must assume an indivisible responsibility for explaining, controlling, and preventing its problem of juvenile delinquency.

JAMES L. MCCARTNEY
223 Stewart Avenue, Garden City, New York

Treatise on Invertebrate Paleontology. Raymond C. Moore, Ed. pt. G, *Bryozoa.* Ray S. Bassler. Univ. of Kansas Press, Lawrence, 1953. 253 pp. Illus. $3.

FOR a century and a half students of fossils have been actively engaged in describing the history of invertebrate life as it is recorded in the rocks of the earth's crust. Although this work is far from complete, the descriptions already written have served as an extraordinarily valuable basis for piecing together and interpreting isolated fragments of the earth's history. In addition to their value as indicators of age and environment, invertebrate fossils extend our knowledge of the course of evolution back some 500 million years. But all who have occasion to work with invertebrate fossils have been hampered by the disordered array of pertinent scientific literature. It is difficult, even for the specialist, to attain any comprehensive view of a major group of invertebrate fossils, except in the relatively rare instances where suitable monographs are available.

The aim of the *Treatise on Invertebrate Paleontology,* ably directed and edited by Raymond C. Moore, is to fill the need for an authoritative, concise summary of knowledge of all groups of invertebrate animals known as fossils. When complete it will contain 24 parts. This is a major scientific undertaking, and the present volume, as the first part of the *Treatise* to be published, will be widely hailed as a landmark in the development of paleontology.

For students of Bryozoa this will be an indispensable handbook. The bulk of the work is devoted to the illustration and brief characterization of 1227 genera. These are grouped into 158 families and 17 suborders, all of which are defined. This description and classification (together with the painstaking resolution of nomenclatorial problems implicit in such a work) constitutes the major contribution of the volume. Other parts of the book are designed to aid the beginner or advanced student in working with Bryozoa. General anatomical data of major groups are briefly summarized.

A comprehensive glossary of morphological terms is provided. Laboratory methods for specimen preparation are discussed. A brief summary of information on the distribution of Bryozoa includes a list of references to the more important publications, tabulated according to geologic age and geographic distribution. A selected list of 129 references serves as a guide to further study. Workers in many disciplines will be grateful to Bassler for this opportunity to view the evolution of a major invertebrate phylum through some 400 million years.

JOHN IMBRIE
Department of Geology, Columbia University

Manual of the Plants of Colorado. H. D. Harrington. Sage Books, Denver, 1954. x + 666 pp. $8.

PROFESSIONAL botanists will welcome this extensive treatment of Colorado plants. It is the only available book that attempts to cover this state. It should be a valuable reference for the central Rocky Mountain area in general.

This manual, written in the terminology of the taxonomist, and without illustrations, is focused on scientific use. It contains a diagnostic key to 117 families and further keys to 693 genera and 2794 species, plus many varieties and subspecies, with consistently detailed descriptions of all these taxons. The author has used the Engler and Prantl sequence for the arrangement of families. He has attempted to bring the nomenclature up to date.

Harrington's manual is notably inclusive in several respects. It covers not only all groups of flowering plants but ferns and conifers as well. Although based almost entirely upon specimens collected or observed, it includes some plants merely reported or expected, and these are listed without descriptions. The author's extensive use of synonyms and their placement after the plant name instead of at the end of the description is helpful. Citations are made to the publication in which the original description of each species was given. The general distribution notes for species are supplemented by Colorado collection data, including altitudinal range. The Colorado distribution was recorded on species' maps not reproduced here owing to the continual change caused by further collecting. A map that divides the state into nine arbitrary regions is used for distribution reference in most cases. The author predicts new additions to the flora, especially in the corners of the state and in the higher altitudes. The western part does not seem to have been covered as exhaustively as the eastern part.

A valuable addition to the manual is the introductory discussion of the vegetative zones in Colorado, by David Costello of the Forest Service, U.S. Department of Agriculture. This not only names the plants in the various ecologic communities but also ties in the influence of man on these communities. A glossary and also a list of new name combinations are included, although no new species is proposed. Harrington claims that about one out of every 30 of his species has not been previously recorded in monographs and manuals.

The reasonable price for this large volume in its attractive cover is undoubtedly due to the photo-offset process. Reading would have been facilitated if the large pages had been divided into double columns. Larger type face for the families and page headings giving the families would also assist the user. It seems regrettable that the extensive material presented here could not have been illustrated by at least simple line drawings, but this would have necessitated a still larger book or a division into volumes.

HARRIET G. BARCLAY
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College Botany. Harry J. Fuller and Oswald Tippo. Henry Holt, New York, rev. ed., 1954. xiv + 993 pp. Illus. + plates. $6.90.

To add to the number of botany textbooks for college students requires real courage and a conviction that the product is of equal or higher quality than other current textbooks. That *College Botany,* first published in 1949 and revised in 1954, is superior in quality is evidenced by its having won a place among the three or four top ranking botany textbooks.

The book may appear to cover more material than can be mastered in a year's course in general botany. This, however, makes it more adaptable for use in varied types of courses than would be possible in a less comprehensive volume. In addition to the usually included subjects of most textbooks, such as the morphology, physiology, and reproduction of seed plants, and a treatment of selected plant groups, there are extensive discussions of soils and fertilizers, variation and heredity in plants, metabolism, growth and irritability, and brief but significant information pertaining to plant evolution, the relationship of plants to their environments, the influence of plants upon human life, and the history and development of botanical science.

At the conclusion of the text there are nearly 1000 topics and questions for study, more than 550 summarizing statements, and 211 suggested readings for interested students. The text is well supplemented and illustrated with numerous halftones, line drawings, two full-page colored plates, and an extensive geologic timetable. Preceding the well-prepared index is a glossary of 734 botanical terms. The use of boldface and italic type to emphasize important words and topics, the selection of numerous and appropriate subchapter headings, the methods of labeling illustrations, and the avoidance of long and complicated sentences make the textbook a most readable and teachable one.

Although the 1954 revision contains a number of changes, there is no major revision or rewriting of topics and chapters. In keeping with the latest discovered facts and accepted theories, the authors have included three changes of special note: on pages 317 and 327 they record the presence of xanthophylls and carotenes associated with chlorophyll rather than a single xanthophyll and carotene, as is given in many textbooks; on pages 924–25 the cedar-hemlock forest of the Pacific Northwest is cited as climax vegetation rather than the Douglas fir forest; and on page 373 one finds that oxygen incorporated into carbohydrates in photosynthesis is derived from carbon dioxide rather than from water. This latter discovery was made possible by the isotope technique.

I recommend this book with real enthusiasm because of its thoroughness, range of subject material, accuracy, and readability.

HOWARD E. MCMINN
Department of Botany, Mills College

Vertebrate Dissection. Warren F. Walker, Jr. Saunders, Philadelphia–London, 1954. ix + 331 pp. Illus. $3.50.

An adequate review of such a book as this could be written only after the book had been used in the laboratory. The book quite naturally deals with the usual laboratory materials and usually in approximately the usual way. Perhaps these facts make it difficult for a reviewer to see the ways in which this manual is different.

The author has selected *Squalus, Necturus, Felis,* and *Lepus* as representative types to illustrate the major anatomical changes in the evolution of vertebrates. However, the protochordates and cyclostomes are also included. The inclusion of these latter forms is evidently part of the "flexibility" mentioned in the preface; flexibility, as used there, means omission of parts for the convenience of shorter courses.

As a comparative anatomist, I wish there had been some discussion of reptiles and birds. Birds, of course, are "off the line of evolution," so to speak, and are only infrequently dissected in beginning courses in comparative anatomy. Reptiles, on the other hand, are squarely in the picture of vertebrate evolution. No doubt, the animals of these classes were omitted because of limitations of space in the book and of time in the laboratory. The author does include two mammals, however.

These remarks are not so much critical of this manual as they are of the content of courses in comparative anatomy. Too frequently, it seems to me, these courses cater to the *desires,* not necessarily the *needs,* of premedical students. This results in the overemphasis on mammals, as the "closest things to man." A broad foundation in comparative anatomy cannot be properly attained under these conditions. Premedical students—and they form the bulk of our enrollment in comparative anatomy—do not in the long run benefit from this plethora of work "to help them in medical school." They need an understanding of structural and cultural values of animals in general, not of mammals alone.

Vertebrate Dissection is designed for the average course in comparative anatomy. The treatment is excellent and the directions are clear. Students who work through this manual and study it will encompass a great deal of knowledge. It fulfills its circumscribed purpose.

HARVEY I. FISHER
Department of Zoology, University of Illinois

Books Reviewed in SCIENCE

6 August

Silicones and Their Uses, Rob Roy McGregor (McGraw-Hill). Reviewed by E. G. Rochow.

Methods of Theoretical Physics, Philip M. Morse and Herman Feshbach (McGraw-Hill). Reviewed by W. V. Houston.

Les Groupes Sanguins Chez les Animaux, R. Dujarric de la Rivière and A. Eyquem (Editions Médicales Flammarion). Reviewed by Ray D. Owen.

Automatic Digital Calculators, A. D. Booth and K. H. V. Booth (Academic Press). Reviewed by Montgomery Phister, Jr.

Microwave Spectroscopy, M. W. P. Strandberg (Wiley; Methuen). Reviewed by Clayton M. Zieman.

Microwave Spectroscopy, Walter Gordy, William V. Smith, and Ralph F. Trambarulo (Wiley; Chapman & Hall). Reviewed by Clayton M. Zieman.

The Metabolism of Algae, G. E. Fogg (Methuen; Wiley). Reviewed by M. H. Adams.

Introduction to Tensors, Spinors, and Relativistic Wave-Equations (*Relation Structure*), E. M. Corson (Hafner). Reviewed by Wallace Givens.

An Illustrated Catalogue of the Rothschild Collection of Fleas (Siphonaptera) *in the British Museum,* vol. I: *Tungidae and Pulicidae,* G. H. E. Hopkins and Miriam Rothschild (British Museum). Reviewed by C. Andresen Hubbard.

Quantitative Organic Analysis via Functional Groups, Sidney Siggia (Wiley; Chapman & Hall). Reviewed by R. L. Shriner.

13 August

Soft Magnetic Materials for Telecommunications, C. E. Richards and A. C. Lynch, Eds. (Interscience; Pergamon). Reviewed by R. M. Bozorth.

The Biochemistry of the Nucleic Acids, J. N. Davidson (Wiley; Methuen). Reviewed by John R. Totter.

Introduction to the Theory of Finite Groups, Walter Ledermann (Interscience; Oliver and Boyd). Reviewed by I. Kaplansky.

Principles of Automatic Controls, Floyd E. Nixon (Prentice-Hall). Reviewed by Jerome Rothstein.

An Rh-Hr Syllabus, Alexander S. Wiener (Grune & Stratton). Reviewed by C. Nash Herndon.

Rh-Hr Blood Types, Alexander S. Wiener (Grune & Stratton). Reviewed by C. Nash Herndon.

Planning Guide for Radiologic Installations, Wendell G. Scott, Chm., Committee on Planning of Radiologic Installations (Year Book Publ.). Reviewed by Vincent W. Archer.

Cancer of the Lung: A Symposium, Johs. Clemmesen, Ed. (Council for International Organisations of Medical Sciences). Reviewed by Sigismund Peller.

Chemistry of Carbon Compounds: Alicyclic Compounds, E. H. Rodd, Ed. (Elsevier). Reviewed by Roderick A. Barnes.

Electroanalytic Chemistry, James J. Lingane (Interscience). Reviewed by Clark E. Bricker.

The Biology of the Cryptic Fauna of Forests, R. F. Lawrence (A. A. Balkema). Reviewed by L. C. Birch.

Synthetische Artbildung, Heribert Nilsson (Verlag CWK Gleerup). Reviewed by Joel W. Hedgpeth.

The Size and Growth of Tissue Cells, Joseph G. Hoffman (Thomas). Reviewed by William R. Duryee.

20 August

Nucleo-Cytoplasmic Relations in Micro-organisms, Boris Ephrussi (Oxford Univ. Press). Reviewed by T. M. Sonneborn.

Nuclear Theory, Robert G. Sachs (Addison-Wesley). Reviewed by Arthur H. Snell.

Encyclopédie Entomologique, Catalogue Illustré des Lucanides du Globe, Texte and Atlas, R. Didier and E. Séguy (Lechevalier). Reviewed by Ashley B. Gurney.

Simultaneous Linear Equations and the Determination of Eigenvalues, L. J. Paige and Olga Taussky, Eds. (National Bureau of Standards). Reviewed by C. R. Cassity.

A Symposium on the Mechanism of Enzyme Action, William D. McElroy and Bentley Glass, Eds. (Johns Hopkins Press). Reviewed by C. B. Anfinsen.

Higher Transcendental Functions, A. Erdélyi, Ed. (McGraw-Hill). Reviewed by R. P. Boas, Jr.

Tables of Integral Transforms, A. Erdélyi, Ed. (McGraw-Hill). Reviewed by R. P. Boas, Jr.

Temperature Measurement in Engineering, H. Dean Baker, E. A. Ryder, and N. H. Baker (Wiley; Chapman & Hall). Reviewed by G. L. Downey.

Advances in Carbohydrate Chemistry, Claude S. Hudson and Melville L. Wolfrom, Eds. (Academic Press). Reviewed by Milton Paul Gordon.

Crystal Growth and Dislocations, Ajit Ram Verma (Academic Press; Butterworths). Reviewed by T. A. Read.

27 August

Infrared Absorption Spectra of Steroids. An Atlas, Konrad Dobriner, E. R. Katzenellenbogen, and R. Norman Jones (Interscience). Reviewed by James E. Page.

The Collected Papers of Stephen P. Timoshenko, (McGraw-Hill). Reviewed by William F. Ryan.

Die Welt der ungewohnten Dimensionen, Arnold Hildesheimer (Sijthoff). Reviewed by Peter G. Bergman.

Progress in Nuclear Physics, Otto R. Firsch, Ed. (Academic Press; Pergamon Press). Reviewed by Herman Feshbach.

Progress in Cosmic Ray Physics, J. G. Wilson, Ed. (Interscience; North-Holland). Reviewed by R. W. Williams.

Flow Properties of Disperse Systems, J. J. Hermans, Ed. (Interscience; North-Holland). Reviewed by Hugh Taylor.

Modulation Theory, Harold S. Black (Van Nostrand). Reviewed by Harner Selvidge.

Physics for Medical Students, J. S. Rogers (Melbourne Univ. Press; Cambridge Univ. Press). Reviewed by Lester I. Bockstahler.

Inorganic Syntheses, John C. Bailar, Jr., Ed. (McGraw-Hill). Reviewed by C. E. Erickson.

Bailey's Text-Book of Histology, Revised by Philip E. Smith and Wilfred M. Copenhaver (Williams & Wilkins). Reviewed by A. J. Ramsay.

Synthesis and Metabolism of Adrenocortical Steroids, W. Klyne, G. E. W. Wolstenholme, and Margaret P. Cameron, Eds. (Little, Brown). Reviewed by A. G. Long.

New Books Received

Field Crop Production. Agronomic principles and practices. Harold K. Wilson and Will M. Myers. Lippincott, Philadelphia, 1954. viii + 674 pp. Illus. $6.

Inorganic Quantitative Analysis. Carroll Wardlaw Griffin. Blakiston, New York, ed. 2, 1954. xvi + 417 pp. Illus. $4.75.

The Black River Studies. John L. Funk *et al.* Univ. of Missouri Studies, Columbia, 1953. 136 pp. Illus. + plates. Paper, $2.50.

Buddhist Texts through the Ages. Edward Conze, Ed. Philosophical Library, New York, 1954. 322 pp. $7.50.

Annual Review of Medicine. vol. 5. Windsor C. Cutting and Henry W. Newman, Eds. Annual Reviews, Stanford, Calif., 1954. ix + 490 pp. $7.

A Textbook of Radar. Staff of the Radiophysics Laboratory, C.S.I.R.O., Australia; E. G. Bowen, Ed. Cambridge Univ. Press, New York, ed. 2, 1954. xiii + 617 pp. Illus. + plates. $8.50.

Animal Control in Field, Farm, and Forest. W. Robert Eadie. Macmillan, New York, 1954. viii + 257 pp. $3.75.

General College Chemistry. M. Cannon Sneed, J. Lewis Maynard, and Robert C. Brasted. Van Nostrand, New York, ed. 2, 1954. vi + 69 pp. Illus. $6.50.

Titanium and Titanium Alloys. John L. Everhart. Reinhold, New York, 1954. v + 184 pp. Illus. $3.

The Science of Chemistry. George W. Watt and Lewis F. Hatch. McGraw-Hill, New York–London, ed. 2, 1954. x + 546 pp. Illus. + plates. $5.50.

Fifty Years of Medicine. Lord Horder. Philosophical Library, New York, 1954. 70 pp. $2.50.

Chemical Engineering in Practice. James I. Harper, Ed. Reinhold, New York, 1954. viii + 140 pp. Illus. $3.

Vertebrate Dissection. Warren F. Walker, Jr. Saunders, Philadelphia–London, 1954. ix + 331 pp. Illus. $3.50.

Public Education and a Productive Society. Horace Mann lecture, 1953. Maurice J. Thomas. Univ. of Pittsburgh Press, Pittsburgh, 1953. 95 pp. $1.

Experimental Electricity for the Beginner. Leonard R. Crow. Scientific Book Publ., Vincennes, Ind., 1953. xvi + 240 pp. Illus. Paper, $2.50.

The Coalfields of Great Britain. Arthur Trueman, Ed. Edward Arnold, London; St Martin's Press, New York, 1954. xi + 396 pp. Illus. + plates. $15.

Semimicro Qualitative Analysis. Edwin O. Wiig, Willard R. Line, and John F. Flagg. Van Nostrand, New York, rev. ed., 1954. viii + 238 pp. Illus. $3.25.

Artificial Fibres. R. W. Moncrieff. Wiley, New York, ed. 2, 1954. xii + 455 pp. Illus. $6.

The Steel Skeleton. vol. I, *Elastic Behaviour and Design.* J. F. Baker. Cambridge Univ. Press, New York, 1954. xi + 206 pp. Illus. + plates. $8.50.

Diagnosis and Improvement of Saline and Alkali Soils. U.S. Salinity Laboratory staff; L. A. Richards, Ed. USDA, Washington, rev. ed., 1954 (Order from Supt. of Documents, GPO, Washington 25). vii + 160 pp. Illus. $2.

Saipan: The Ethnology of a War-Devastated Island. Alexander Spoehr. Chicago Natural History Museum, Chicago, 1954. 383 pp. Illus. Paper, $5.

The Flemish Masters. Horace Shipp. Philosophical Library, New York, 1954. 128 pp. + 39 plates. $6.

Wartime Psychiatry. A compendium of the international literature. Nolan D. C. Lewis and Bernice Engle, Eds. Oxford Univ. Press, New York, 1954. vi + 952 pp. $15.

The Flood Control Controversy. Big dams, little dams, and land management. Luna B. Leopold and Thomas Maddock, Jr. Ronald Press, New York, 1954. xiii + 278 pp. $5.

Preservation and Transplantation of Normal Tissues. A Ciba Foundation symposium. G. E. W. Wolstenholme and Margaret P. Cameron, Eds. Little, Brown, Boston, 1954. xii + 236 pp. Illus. + plates. $6.

Yeast Technology. John White. Wiley, New York, 1954. xvi + 432 pp. Illus. + plates. $8.

Histopathologic Technic and Practical Histochemistry. rev. ed. of *Histopathologic Technic.* R. D. Lillie. Blakiston, New York, 1954. ix + 501 pp. $7.50.

Zoo-Safari. Bericht der Deutschen Zoologischen Ostafrika-Expedition 1951–52. Erwin Lindner. E. Schweizerbart, Stuttgart, 1954. ix + 139 pp. Illus. + plates. DM. 15.40.

Theory of Games and Statistical Decisions. David Blackwell and M. A. Girshick. Wiley, New York; Chapman & Hall, London, 1954. xi + 355 pp. Illus. $7.50.

Proceedings of the Second International Congress on Rheology. V. G. W. Harrison, Ed. Academic Press, New York; Butterworths, London, 1954. ix + 451 pp. Illus. $10.

The World of Learning, 1954. Europa Publ., London, ed. 5, 1954. 1030 pp. $17.50.

An Introduction to Climate. Glenn T. Trewartha. McGraw-Hill, New York–London, ed. 3, 1954. vii + 402 pp. Illus. + maps. $7.

A Philosophical Study of the Human Mind. Joseph Barrell. Philosophical Library, New York, 1954. xii + 575 pp. Illus. + plates. $6.

Principles of Geomorphology. William D. Thornbury. Wiley, New York; Chapman & Hall, London, 1954. ix + 618 pp. Illus. $8.

Science and the Common Understanding. J. Robert Oppenheimer. Simon and Schuster, New York, 1954. 120 pp. $2.75.

Radiation Biology. vol. I, *High Energy Radiation,* pts. I and II. Alexander Hollaender, Ed. McGraw-Hill, New York–London, 1954. ix + 1265 pp. Illus. $17.50.

College Textbook of Physics. Arthur L. Kimball. Rev. by Alan T. Waterman. Holt, New York, ed. 6, 1954. xii + 942 pp. Illus. $7.95.

The Compleat Strategyst. Being a primer on the theory of games of strategy. J. D. Williams. McGraw-Hill, New York–London, 1954. xiii + 234 pp. Illus. $4.75.

Geography of North America. George J. Miller, Almon E. Parkins, and Bert Hudgins. Wiley, New York and Chapman & Hall, London, ed. 3, 1954. xi + 664 pp. Illus. $7.50.

New Biology. No. 16. M. L. Johnson, Michael Abercrombie, and G. E. Fogg, Eds. Penguin Books, Baltimore, 1954. 135 pp. Illus. + plates. Paper, $0.50.

Freedom against Itself. Clarence K. Streit. Harper, New York, 1954. xviii + 316 pp. $3.75.

Wave Motion and Vibration Theory. Proc. of Symposium in Applied Mathematics of the American Mathematical Society. vol. V. Albert E. Heins, Ed. McGraw-Hill, New York–London, 1954. v + 169 pp. Illus. $7.

Representative Chordates. A manual of comparative anatomy. Charles K. Weichert. McGraw-Hill, New York–London, 1954. vii + 204 pp. Illus. $3.50.

LETTERS

The Trumpeter Swans

Just a word of deep appreciation for the beautiful and appealing photograph of the trumpeter swans on the July cover. Some years ago I participated in a campaign for the protection of this remarkable bird, which was saved from extinction through the efforts of the authorities of one of the states.

In the recent annual report of the National Parks, the director, Conrad Wirth, states that the number of these swans counted in an aerial survey over Yellowstone Park—74 adults and 17 cygnets—was "approximately the same as for several years." Hence, one may assume that this species is at least holding its own.

C. M. Goethe

Sacramento 14, California

Parapsychology and Dualism

I am greatly interested in your leading article of July 1954, entitled "Parapsychology and dualism." After enumerating telepathy, clairvoyance, psychokinesis, and precognition as being subjects of experiment with parapsychologists, the author says:

> Although some careful skeptics are still not satisfied with some of the experiments, for the present it is assumed that, under adequate experimental control, there is a considerable body of valid phenomena of the kind outlined in the preceding section and referred to by Rhine's school as psi phenomena. [p. 2]

But while to Rhine's school these admitted results of experiments, carefully made, seem now and forever outside the realm of what the author defines as "mechanistic," the author puts them in the limbo of the as yet unexplainable in that realm, but feels convinced that such of them as are sound will eventually fall therein.

This distinction is of great interest. For all religions, with whatever of good has come from the adherence of multitudes to them, depend fundamentally on the belief that there is a second realm, not "mechanistic." On the other hand the present practice of communism in Russia, which is the dreaded enemy of our time, is based on a frankly "mechanistic" philosophy.

Charles G. Abbot

Smithsonian Institution, Washington 25, D.C.

I greatly regret that my attempt at analysis is considered as giving aid to the forces of darkness; since Dr. Abbot has raised the point, I wish to dissent from what he seems to imply by the correlation. As Dr. Grünbaum points out [*Sci. Monthly* **79**, 13 (July 1954)], a theory may be false *and* misappropriated for dubious social ends but not false *because* so misappropriated.

I do not believe that the good in religions necessarily depends on dualistic belief; nor do I believe that the evils of communism are based on the mechanistic philosophy. Values are not implicit in, or excluded by, either dualism or mechanism. Man may aspire and give loyalty to ideals of the good and true measured against a rational single standard as well as against a dual standard.

Roland Walker

Department of Biology, Rensselaer Polytechnic Institute, Troy, New York

A Walt Disney in Ancient Egypt

Everyone likes to speculate about the origins of inventions and customs. When did man first make fire, and how did he learn? When was the first wheel invented? Who first thought of making a mold for casting metal? What thoughts and experiments could have resulted in making a serviceable alloy from copper and tin? Who first felt the desire to draw pictures?

Some of the most realistic early drawings of wild animals were done in color on the walls of caves in France and Spain. The artists were men of the Old Stone Age, a period of indefinite length, which occurred in Europe many thousands of years ago. But these cave pictures are not humorous. In fact the suggestion has been made that they played a part in magical rites connected with hunting, which was the principal means of livelihood of Stone Age man.

When did an artist first introduce humor into his animal drawings? Anthropologists are always reluctant to assert that they have discovered the very first example of any invention, process, or custom, because the general result of further research is to push the time of origin far back into periods long before the invention of paper and writing.

So far as humor in the sketching of animals is concerned, I believe that the charming efforts of Walt Disney and other artists were anticipated by about 3000 years. I have made a copy of a drawing of animals in comic situations sketched by an Egyptian artist of whom we know very little. Even his name is not on record.

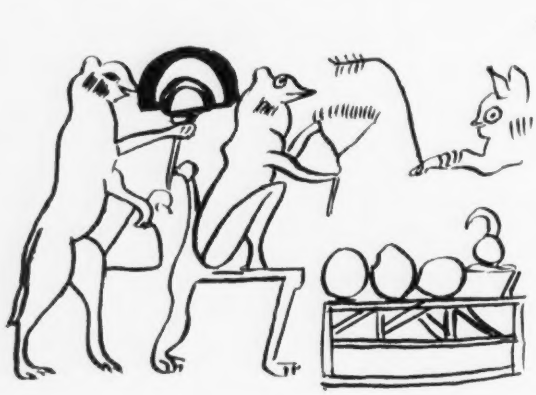
At an early date the Egyptians had discovered the art of making a kind of paper from a reed named papyrus that grew extensively in the marshes of the Nile River, especially in the delta. A bundle of unused papyrus was found in a tomb dated about 3000 B.C., and on material of this kind our Egyptian humorist recorded his impressions of animals placed in quaint situations.

He has based his animal antics on a principle that is followed today in motion pictures and comic strips; the humor lies in the sharp contrast between the animals' activities in the pictures and their habits in real life. In No. 1 a mouse is seated on a chair holding a fan in its paw, while another mouse stands behind, holding a fan. This, of course, is a representation of an im-

portant person with a slave to fan him during meals, according to Eastern custom. The meal consists of pumpkins, and the one with the curved neck is commonly grown today. On the right, a cat, implacable enemy of the mouse, is offering a symbol of peace, an olive branch. The artist has followed a common Egyptian custom of drawing in profile; he has given the chair no perspective.

Picture No. 2 shows our old friend the fox peacefully employed in herding young goats, animals the fox is much more likely to regard as dainty suppers. The fox-herdsman carries his baggage on a stick, and the fox who brings up the rear is playing on pipes that probably represent the ancient wind instrument known as Pan pipes, of very wide geographic distribution.

1



2

3

Evidently the cat of tabby type was known quite early in Egypt, for in No. 3 the cat who carefully marches the geese has markings that represent those of our modern tabby.

T. G. H. James of the British Museum, London, kindly answered my questions as far as possible and said there was no objection to use of the drawing. Although the name of the artist is unknown, it is established that the papyrus was decorated about the year 1100 B.C., and that the artist was employed in a cemetery at Thebes. Moreover, this papyrus is not the only example of its kind, for copies of similar scenes have been made by French archeologists.

The artist whose work I have copied, and other designers of similar ability, were employed in activities that daily associated them with death and burial. Partly as required labor in decorating tombs, and perhaps for personal amusement as well, they created these quaint animal antics. How little they thought that their creative ability anticipated an American million-dollar industry by about 3000 years.

WILFRID D. HAMBLY

Curator of African Ethnology (retired),
Chicago Natural History Museum

Logic or Beauty?

I would like to reply to a criticism by A. Lowinger [*Sci. Monthly* **78,** 399 (June 1954)].

It is true that the ultimate goal of theoretical physics is merely to get a set of rules in agreement with experiment. But it has always been found that highly successful rules are highly beautiful and ugly rules are of only restricted use. In consequence, physicists generally have come to believe in the need for physical theory to be beautiful, as an overriding law of nature. It is a matter of faith rather than of logic.

When quantum mechanics was first discovered, its beauty led to the great enthusiasm with which it was immediately taken up by the world's physicists. Only after years of patient working out of details did its wonderful agreement with experiment gradually get revealed.

Present-day theories about the nature and stability of the elementary particles have little beauty, so one should not be surprised at their success being limited. One may confidently expect them to be only a passing phase in the progress of physics and to be superseded in the course of time by a new theory whose first claim to attention will be its beauty.

The situation is well illustrated by a story that was told to me by E. Schrödinger. In the early 1920's, when physicists had only the Bohr theory of the atom to work with, Schrödinger got the idea that it should be possible to obtain the energy levels of an atomic system by solving an eigenvalue problem. He worked on this idea for a number of years and eventually, aided by de Broglie's theory of plane waves associated with the motion of free particles, thought of a beautiful equation with the operators $\delta/\delta t$ and $\delta/\delta x$ taking the place of the energy and momentum.

He immediately applied this equation to the hydrogen atom, treating the motion of the electron relativistically. He worked out the eigenvalues and found that they did *not* agree with the observed energy levels. The discrepancy was due to the fact that in those days people did not know about the spin of the electron, and Schrödinger had not taken it into account. Schrödinger concluded that his equation was a failure and was very dejected about it and abandoned it.

Several months later he went back to it and noticed that if he was less ambitious and contented himself with working nonrelativistically, the results agreed with experiment in nonrelativistic approximation. He then wrote up the nonrelativistic theory and published it, and so the famous Schrödinger wave equation came to be presented to the world.

The moral of the story is that one should have faith in a theory that is beautiful. If the theory fails to agree with experiment, its basic principles may still be correct and the discrepancy may be due merely to some detail that will get cleared up in the future.

P. A. M. DIRAC

Cambridge University, England

I am interested in the complete assurance of Armand Lowinger's denial: "The beauty of the theory had absolutely nothing to do" with its acceptance by "the physicists." How does he know this? How can anyone know all the factors underlying the acceptance of a theory by any physicist, let alone by "the physicists"?

Even if indeterminacy were not the hard pill it is for a physicist, so that its acceptance must be what we might call a compelled acceptance—owing to the compulsion of experimental fact and logic combined—other and subjective factors can hardly be excluded, unless one has access to those wider judgments about the constitution of nature that play in the mental background of that various group of persons, "the physicists."

I am also interested in "the beauty of the theory." What precisely is meant by this phrase? It occurs to me that one needs to know this meaning before dogmatically asserting its irrelevance. My impression has been that "beauty" in mathematics is akin to "power"; an "elegant" demonstration, for example, is one that accomplishes its deduction with a minimum of apparatus. If this impression is correct, the "beauty" of a theory would be akin to the simplicity of its most general formulas and to the clarity of the lines from these formulas to the experimental situation. Am I right or wrong in supposing that the compactness of Einstein's formula for the relationship of mass and energy had *something* to do—after experiment had brought some confirmation—with its fairly general acceptance?

We need not go back to the time when Kepler found a few pleasantly neat formulas for planetry movements, actuated by a preconceived belief that nature would do things on a plan having a certain "harmony" of its own. Insofar as "the physicists" accepted Kepler's laws, they did so because of the laws' approximate verifiability, not because of any beauty in the theory itself. Yet if we could get at the full psychology of those who accepted Kepler, or of those who later accepted Newton's "powerful" unification of theory, I should not think less of them as scientists if they had been to some extent impressed by the "beauty" of the thing, as an additional item of confirmation. Would you?

ERNEST HOCKING

Alford Professor Emeritus (Philosophy), Harvard University

In Defense of the Passive Voice

The passive voice of the verb has been scolded lately by commentators on technical writing. May I use it occasionally (as in the preceding sentence) and say a few words in its defense? It is possibly in need of a little moral support.

Sometimes the passive voice has the merit of placing the real subject at the beginning of the sentence, where it belongs. Would the first sentence of this note be any more effective if it read: Commentators on technical writing have lately been scolding the passive voice of the verb? In comparing the two sentence forms, bear in mind that the subject to be discussed is not *commentators* but *the passive voice.*

Here is another example: (i) Spectrograms of Nova Whosis were obtained at Mount Wilson every night in July. (ii) I obtained spectrograms of Nova Whosis at Mount Wilson. . . . I think that version (i) is usually preferable unless the writer wishes to emphasize that he, not somebody else, took the spectrograms.

PAUL W. MERRILL

Mount Wilson and Palomar Observatories, Pasadena 4, California

Not Mark Twain but Warner

According to Burton Stevenson's *Home Book of Quotations* (Dodd, Mead, ed. 6, 1952), it was Charles Dudley Warner, in the *Hartford Courant,* who said of the weather, "everybody talks about it but nobody does anything"—a remark often attributed to Mark Twain [see, for example, *Sci. Monthly* **78**, 339 (1954)].

RICHARD S. WORMSER

22 West 48 Street, New York 36

ASSOCIATION AFFAIRS

PROGRAMS OF THE BERKELEY MEETING, 26–31 DECEMBER 1954

Advance indications last spring that this year's 121st meeting of the Association at the University of California at Berkeley would be the largest diversified scientific meeting ever held on the Pacific Coast are being realized. In addition to the programs of the Association and its 18 sections, 59 societies and other scientific organizations have scheduled more than 300 sessions; another 30 societies will cosponsor appropriate symposiums. It will also be one of the largest meetings in the long history of the AAAS, now in its 107th year.

The size of the attendance, however, is only one aspect of a successful meeting. It assures that one will find friends and coworkers in one's specialty also attending the meeting, but the physical arrangements, the quality of the programs, the opportunities to meet scientific leaders from other parts of the world, the impact of new concepts, and the general stimulation of informal discussions are all factors of importance. In these respects, too, the Berkeley meeting should be particularly satisfactory.

The campus of the University of California, which is remarkably compact for so large an institution, has an adequate number of well-equipped session rooms. With the exception of the National Geographic Society's annual lecture and accompanying film, to be held in the Berkeley High School auditorium, and the sessions of Section P at the Hotel Claremont, all sessions for papers, symposiums, and evening addresses will be held on the campus. Center of the meeting is the large Gymnasium for Men, which will house the one AAAS Registration-Information Center, the Visible Directory of Registrants, the AAAS Science Theatre, and the AAAS Annual Exposition of Science and Industry—with 114 booths. Directly across the street, the University of California Alumni Association has generously made available its new luxuriously furnished Lounge for all who attend the meeting. A variety of university laboratories and museums will be of interest to scientists in all principal fields.

An unusual variety of housing accommodations are available, ranging from the university dormitories—two persons to a room at the nominal charge of $2 each the first night, $1 per night thereafter—and International House, to hotels and motels. Although single rooms near the campus are now scarce, there are ample accommodations in Berkeley, at moderate rates, if colleagues will share a twin-bedded room. Four university cafeterias grouped in one central location will be particularly convenient for luncheons and dinners. The California Academy of Sciences in Golden Gate Park, San Francisco, has issued an invitation to all visitors to tour the Science Museum with its science library and research departments, the Steinhart Aquarium, and its new Morrison Planetarium (admission free to all registrants).

The 121st meeting will have a decidedly international aspect resulting from the concurrence of the Third Berkeley Symposium on Mathematical Statistics and Probability, a 4-day International Conference on Animal Venoms, and the program of the International Union for the Study of Social Insects. The American Physical Society has arranged for the presence of Fritz Zernike, 1953 Nobel prize winner from the Netherlands. Besides the scientists from Europe and the Eastern Hemisphere, a heavy representation from Hawaii, the Philippines, and other Pacific areas is anticipated. In addition to the annual Academy Conference, made up of delegates of the 42 academies of science affiliated with the Association, there will be the recurrent Conference on Scientific Editorial Problems and the Conference on Scientific Manpower. The following data on participating organizations and programs indicate the scope and quality of the meeting.

AAAS as a Whole

General symposium, *Science and Society*: I. "Natural resources: power, metals, food," arranged by Louis B. Slichter; II. "Population problems," by Curt Stern; III. "Impact of science on society," by Roger R. Revelle, 27–29 Dec.

AAAS presidential address by E. U. Condon, and reception, 28 Dec.

AAAS Council meetings, 27, 30 Dec.

Biologists' smoker (for all registrants), 30 Dec.

(Note: For brevity, except for certain joint programs, official cosponsorships have been omitted in the following tabulation.)

A—Mathematics

Section A and the *American Mathematical Society*—Five sessions for contributed papers and invited addresses, arranged by R. E. Langer and D. H. Lehmer, 30–31 Dec.

American Statistical Association and *Pacific Coast Committee on Statistics of the Social Science Research Council*—Arranged by Maurice I. Gershe

son: "Contributions of statistics to physics," and "Regional indexes of business activity," 27 Dec.; "Regional unemployment estimates," 28 Dec.

Institute of Mathematical Statistics—Sessions for papers, arranged by Leo Katz and Evelyn Fix, 26 Dec.

Third Berkeley Symposium on Mathematical Statistics and Probability, sponsored by the Statistical Laboratory of the University of California, arranged by Jerzy Neyman, director, and Elizabeth L. Scott—"Mathematical statistics" and "Statistics in biology and genetics," 27 Dec.; "Statistics in medicine and public health," 28, 29 Dec.; "Mathematics" and "Statistics in psychology," 30 Dec.; "Statistics in industrial research" and "Statistics in physics," 31 Dec.

There will be a joint beer party and dinner for all statisticians and mathematicians.

B—Physics

American Physical Society—About 30 sessions of contributed and invited papers, on nuclear physics, arranged by A. C. Helmholz; high-energy machines, by E. J. Lofgren; solid-state and microwave physics, by Charles Kittel; electron physics, by Daniel Alpert and Leonard B. Loeb; theoretical physics, by Edward Teller; astrophysics, by Otto Struve; and instrumentation, arranged by Joseph Kaplan and W. A. Nierenberg, 28–30 Dec; physicists' dinner, cosponsored by *Section B* and *Sigma Pi Sigma,* 30 Dec.

American Meteorological Society—Symposium, "Climatology," 28 Dec.; and six or more sessions for contributed papers, arranged by Kenneth C. Spengler.

C—Chemistry

Section C and the *American Chemical Society, California Section*—Symposium, "Petroleum chemistry," arranged by John W. Givens, 27 Dec.; symposiums, "Viruses" and "Nucleic acids," arranged by Wendell M. Stanley, 28 Dec.; symposium, "Chemistry in the harnessing of biological resources," arranged by J. C. Lewis, 29 Dec.; sessions for contributed papers, 26, 30 Dec.; chemists' dinner and vice presidential address by Wendell M. Latimer, 29 Dec.

Alpha Chi Sigma—Chemists' luncheon, 28 Dec.

Pacific Southwest Association of Chemistry Teachers—Sessions for contributed papers, arranged by Mother Agnes Schmit, 28 Dec.

D—Astronomy

Section D and the *Astronomical Society of the Pacific*—Symposium, "Nebular red shifts," 27 Dec.; symposium, "Spatial distribution of galaxies," 28 Dec., and symposium, "Distribution of stars in the Russell-Hertzsprung diagram," 29 Dec., arranged by Louis G. Henyey, Jerzy Neyman *et al.*; sessions for contributed papers, 27–29 Dec.; society dinner and vice presidential address, "The new science of radio astronomy," by Bart J. Bok, 29 Dec.; trip to Lick Observatory, 30 Dec.

Meteoritical Society—Sessions for contributed papers, arranged by John A. Russell, 28, 29 Dec.

E—Geology and Geography

Section E—Contributed papers in general geography, cosponsored by the *Association of American Geographers, Pacific Division* and the *Association of Pacific Coast Geographers,* 28 Dec.; symposium, "Earth sciences from the air," arranged by Parker D. Trask, and geologists' smoker and vice presidential address by Meredith F. Burrill, 29 Dec.; contributed papers in general geology, cosponsored by the *Geological Society of America,* 30 Dec.

Arctic Institute of North America—Sessions for papers on arctic science, arranged by John L. Mohr and Joseph T. Flakne, jointly with the Western Society of Naturalists, 28–30 Dec.

Division of Mines, State of California—Mineral exhibit of the Division and large relief map of California, in Ferry Building, San Francisco, open daily, 9 A.M. to 4 P.M. Geologic field trip of the Bay Area and central coast ranges, arranged by Olaf P. Jenkins and Gordon B. Oakeshott, 28 Dec.

National Geographic Society—Annual illustrated lecture with AAAS, 27 Dec.

National Speleological Society—Session for papers and business meeting, arranged by Burton Faust, 27 Dec.

F—Zoological Sciences

International Conference on Animal Venoms, arranged by Nandor Porges, jointly sponsored by *Sections F* and *N*—10 or more sessions on all phases of all groups of venomous animals, 27–30 Dec.

Cooper Ornithological Society—Session for contributed papers, arranged by Frank A. Pitelka, 29 Dec.

Herpetologists League—Arranged by Angus M. Woodbury and Joseph Gorman: symposium, herpetologists' dinner, 29 Dec.; and sessions for contributed papers, 29, 30 Dec.

International Union for the Study of Social Insects—Symposium, arranged by T. C. Schneirla, 28 Dec.

Society of Systematic Zoology and *SSZ Pacific Section*—Arranged by R. E. Blackwelder and Robert C. Stebbins: council meeting, symposium, "Animal courtship," film and public lecture on African animals, 27 Dec.; session for papers, open house at California Academy of Sciences, council meeting, 28 Dec.; business meeting, panel on museums, symposium, "Desert evolution," 29 Dec.; session for entomological papers, jointly with the *Pacific Coast Entomological Society,* symposium, "Australian mammals," 30 Dec.; zoologists' dinner and vice presidential address by H. W. Stunkard, cosponsored by *Section F,* 30 Dec.

FG—Biological Sciences

American Society of Limnology and Oceanography—Sessions for contributed papers and business meeting, 28–30 Dec.; symposium, "Recent advances in biological oceanography," arranged by Robert W. Hiatt, 30 Dec.

American Society of Naturalists—Symposium, 29 Dec.

Beta Beta Beta—Special meeting of Western Region, arranged by Lloyd M. Bertholf, 30 Dec.

Biometric Society, WNAR—Sessions for papers, arranged by Douglas G. Chapman and Elizabeth Vaughan, 27 Dec.

Ecological Society of America and *ESA Western Section*—Arranged by Russell K. LeBarron and E. C. Stone: sessions for contributed papers and ecologists' dinner, 27 Dec.; session for papers and symposium, "Dew as an ecological factor," 28 Dec.; field trip to Muir Woods (redwoods) and Stinson Beach State Park, 29 Dec.

National Association of Biology Teachers—Arranged by Brother H. Charles: business meetings, 27, 28 Dec.; films and panels, "Biology for pleasure" and "Biology for living," 28 Dec.; panel, "Biology for survival," NABT luncheon, and panel, "Biology laboratory problems," 29 Dec.; field trip to Muir Woods and Stinson Beach State Park, 30 Dec.; business meetings, 30, 31 Dec.; conservation education, 31 Dec.

Society of General Physiologists—Sessions for invited papers, arranged by John Buck, 30 Dec.

Western Society of Naturalists—Symposium, "Natural resources of the West," arranged by Arthur C. Giese, and invited papers in natural history, arranged by John L. Mohr, 27 Dec.; invited papers on "Photochemical effects in biological and biochemical systems," arranged by A. D. McLaren, 27, 28 Dec.; contributed papers in parasitology and business meeting, 28 Dec.; symposium, "The cell," arranged by Daniel Mazia and Roger Y. Stanier, invited papers on "Teaching problems in biology," contributed papers in histology-embryology, contributed papers in ecology, naturalists' banquet and presidential address, 29 Dec.; contributed papers in physiology, 30 Dec.

G—Botanical Sciences

Section G and *Botanical Society of America, Pacific Division*—Symposium on plant anatomy, arranged by Adriance Foster, and botanists' dinner and vice presidential address by Stanley A. Cain, 30 Dec.; cosponsorship of symposiums of following G societies.

American Phytopathological Society, Pacific Division—Arranged by William B. Hewitt: conference on control of soil microorganisms, 27 Dec.; symposium, "Crop sequence and plant disease control," and phytopathologists' banquet, 28 Dec.; demonstrations, sessions for invited papers on soil-borne virus diseases, sessions for contributed papers and symposium, "Mode of survival of plant pathogens in the soil," 29 Dec.

American Society of Plant Physiologists, Western Section—Symposium on mineral nutrition, arranged by Perry R. Stout, 29 Dec.

Mycological Society of America—Symposium, "Physicochemical control of structural differentiation in the fungi," arranged by Ralph Emerson, 28 Dec.; session for contributed papers, 29 Dec.

H—Anthropology

Section H—Arranged by Gabriel Lasker: symposium, "Indians of western North America" and session for contributed papers, 28 Dec.; symposium, "Culture change in the Pacific region," and session for contributed papers, 29 Dec.; symposium, "Impact of industrialism," and session for contributed papers on demography and human evolution, 30 Dec.

I—Psychology

Section I—Sessions for invited papers on learning, arranged by John P. Seward, and on highway safety, arranged by T. W. Forbes, 28 Dec.; session for invited papers on brain function, arranged by Donald B. Lindsley, and on human engineering, arranged by Arnold Small, and vice presidential address by Donald B. Lindsley, 29 Dec.

Society for Research in Child Development—Session for contributed papers, arranged by Lois Meek Stoltz, 27 Dec.

Western Psychological Association—Arranged by Rheem F. Jarrett: symposiums, "Present status of psychoanalytic theory" and "Sex differences in personality and intellectual development," 27 Dec.; symposium, "Perception," 30 Dec.

K—Social and Economic Sciences

Section K, Western Economic Association et al.—Panels, "Regional economic analysis," arranged by David Revzen, and "Cost-benefit analysis for natural resource development," arranged by M. L. Upchurch, 28 Dec.; panels, "Trends in urbanization," arranged by Calvin F. Schmid, and "Structure of cities," arranged by Leonard Broom, 29 Dec.; panel, "Organization of research for western development," arranged by Ernest A. Engelbert and social sciences dinner and vice presidential address, "International consequences of scientific research," by John B. Condliffe, 30 Dec.

AAAS Committee for Social Physics—Arranged by Stuart C. Dodd: sessions for invited papers on "Diffusion theory," "Human values measurement," and "Isomorphisms," 27, 28 Dec.

National Academy of Economics and Political Science with the collaboration of *Pi Gamma Mu*—Symposium, "National defense against atomic attack," arranged by Donald P. Ray, 27 Dec.

Society for the Advancement of Criminology—Symposium, "Education in criminology," arranged by

William Dienstein, business meeting, and dinner, 28 Dec.

(Note: The programs of the *American Statistical Association* and other groups in statistics will be found under A—Mathematics.)

L—History and Philosophy of Science

Section L and *Philosophy of Science Association*—Symposiums, "International aspects of the philosophy of science," "National loyalty and security in relation to scientific idealism," and "Meaning of concepts in scientific theories," arranged by Raymond J. Seeger; symposium, "Scientific preparation for the health profession," arranged by D. Bailey Colvin; and a symposium, "Induction," arranged by E. W. Barankin, and sessions for contributed papers, 27–30 Dec.; and vice presidential address by Chauncey D. Leake, 30 Dec.

M—Engineering

Section M—Symposium, "Air pollution," arranged by Lauren B. Hitchcock, 29 Dec.; symposium, "Prosthetic devices," arranged by Eugene F. Murphy, 30 Dec.

N—Medical Sciences

Section N—Four-session symposium, "Physiology of growth—normal and abnormal," arranged by Howard R. Bierman, and including the vice presidential address of Charles Huggins, 27, 28 Dec.

Alpha Epsilon Delta—Arranged by Maurice L. Moore: symposium, "Preparation for medical education in the liberal arts college," luncheon, and round-table discussions on premedical education, 30 Dec.

American Academy of Forensic Sciences—Symposium on forensic sciences, arranged by Ralph F. Turner, 29 Dec.

American Association of Hospital Consultants—Symposium on hospital problems, arranged by Jack Masur, 30 Dec.

American Psychiatric Association—Symposium on recent advances in psychiatry, arranged by Jacques S. Gottlieb, 30 Dec.

Bay Counties Veterinary Medical Association—Arranged by Joseph M. Arburua: sessions for invited papers on animal diseases, luncheon, and social hour, 28 Dec.

Donner Laboratory of Biophysics, University of California—Symposium, "Cell biophysics," arranged by Cornelius A. Tobias, 30 Dec.

Pacific Slope Biochemical Conference—Sessions for submitted papers, arranged by David M. Greenberg, 30 Dec.

Society for Experimental Biology and Medicine, Southern California and Pacific Coast Sections—Symposium, "Adrenal-pituitary relationships," arranged by Robert H. Dreisbach, and session for contributed papers, 28 Dec.

Society of American Bacteriologists, Southern California and *Northern California-Hawaii Branches*—Sessions for contributed papers, arranged by Frances A. Hallman and H. J. Phaff, 28 Dec.

Section Nd—Dentistry

Section Nd, American College of Dentists, American Dental Association and *North American Division of International Association for Dental Research*—Symposium, "Growth and development," arranged by Wendell L. Wylie, luncheon, symposium, "Radiation hazards in the dental office," arranged by Gordon M. Fitzgerald, 28 Dec.

Section Np—Pharmacy

Section Np, American Association of Colleges of Pharmacy, American College of Apothecaries, American Drug Manufacturers Association, Scientific Section of American Pharmaceutical Association, American Pharmaceutical Manufacturers Association, and *American Society of Hospital Pharmacists*—Arranged by Donald C. Brodie: symposium, "The pharmacy internship" and three sessions for contributed papers, 28, 29 Dec.

O—Agriculture

Section O—Symposiums, cosponsored by the *Western Society of Soil Science*, "Soil management problems in western agriculture," arranged by H. B. Cheney, and "Water supplies and irrigation," arranged by F. J. Veihmeyer, 27 Dec.; symposiums, "Seed production in the western states," arranged by Frank G. Parsons, and "Problems in horticultural crops," arranged by John H. MacGillivray, 28 Dec.

P—Industrial Sciences

Section P—Arranged by Allen T. Bonnell: roundtable sessions on "Basic research—contributions of government, industry, and the university," and luncheon, 30 Dec.

Q—Education

Section Q—Sessions for contributed papers, 27–29 Dec.; concurrent sessions, jointly with *American Educational Research Association*, 27 Dec.; symposium on visual performance, arranged by N. Franklin Stump, 28 Dec.; symposium, jointly with the *International Council for Exceptional Children*, on the exceptional child, and vice presidential address by George C. Kyte, 29 Dec.

AAAS Cooperative Committee on the Teaching of Science and Mathematics—Arranged by John R. Mayor: papers, panel, and evening lecture on "Science teachers for tomorrow," 30 Dec.

National Science Teachers Association—Arranged by Robert Stollberg: panels, "Role of science in the education of youth," "Keeping up to date in science," informal luncheon, buffet supper and social

mixer, 27 Dec.; work-discussion groups on "Some problems science teachers must solve," 28 Dec.; session on "Recent research in science" and six concurrent demonstration groups on "Here's how I do it," 29 Dec.

X—Science in General

Academy Conference—Business meeting, panel discussion of Academy of Sciences problems, and Academy Conference Dinner, 27 Dec.; Junior Scientists' Assembly—a special program for young scientists—at San Francisco State College, 29 Dec.

American Nature Study Society—Arranged by Ruth E. Hopson: sessions, 26, 27 Dec.; annual meeting and showing of kodachromes, 27 Dec.; joint sessions with NABT, 28, 29 Dec.; field trip to Muir Woods and Stinson Beach State Park, 30 Dec.; sessions, 31 Dec.

Conferences on Scientific Editorial Problems—Sessions for invited papers and discussion, arranged by Marian Fineman, 29 Dec.

Conference on Scientific Manpower, cosponsored by the *Engineering Manpower Commission, National Research Council, National Science Foundation,* and the *Scientific Manpower Commission*—Sessions for invited papers and discussion, arranged by Thomas J. Mills, 28, 29 Dec.

Honor Society of Phi Kappa Phi—Special program, 29 Dec.

National Association of Science Writers—Symposium, arranged by John Pfeiffer, 28 Dec.

Pacific Science Board—Public Lecture by Alexander Spoehr, 30 Dec.

Scientific Research Society of America—Annual convention and luncheon, annual RESA address and award of William Procter prize, 30 Dec.

Society of the Sigma Xi—Annual Sigma Xi address by Edwin M. McMillan, 29 Dec.; annual convention and luncheon, 30 Dec.

United Chapters of Phi Beta Kappa—Annual address, by Conway Zirkle, 27 Dec.

AAAS Travel Arrangements

A page on AAAS Travel Arrangements has appeared at intervals in the advertising sections of both journals of the Association; a gratifying number of coupons has been received and acknowledged. Group flights to Oakland or San Francisco, 26 Dec., and return, 31 Dec.—either by chartered *first-class* DC 6B or 7 planes (at rates approaching those for coach flights) or as extra sections of existing flights are in the final stage of arrangements. (As on all scheduled flights, individual insurance may be carried.) *All persons east of the Rockies who plan to fly to the meeting and have not sent in a AAAS Travel Coupon are urged to do so immediately.*

RAYMOND L. TAYLOR
Associate Administrative Secretary

A NEW HOME FOR THE AAAS

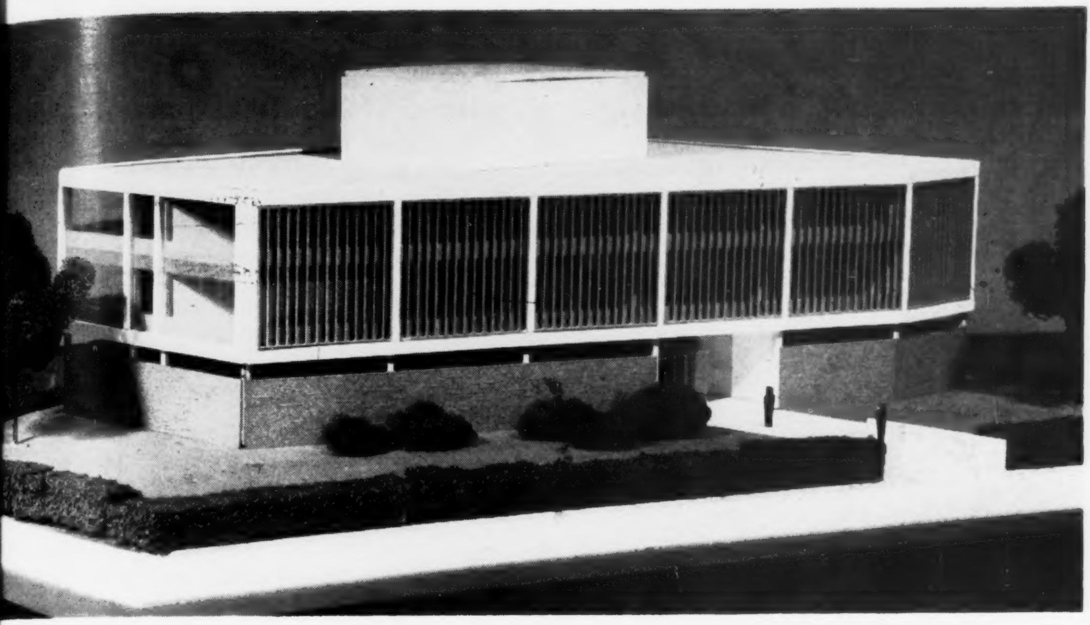
ON 4 August the District of Columbia Board of Zoning Adjustment gave the AAAS permission to erect a headquarters building on the Association's property at 1515 Massachusetts Ave. NW in Washington. Members who have built their own homes and those who remember Mr. Blandings' dream house—whose author, by the way, gained additional laurels in 1953 in winning one of the AAAS–George Westinghouse Science Writing Awards—will have a sympathetic feeling both for the excitement of the occasion and for the fact that a good many headaches lie between permission to build and actual occupancy.

The model pictured here was built to show the Board of Zoning Adjustment the size and general character of the proposed building. The building itself will not necessarily match the model in all respects, for detailed drawings have not yet been prepared. The size, however, is definite. There will be three stories and a basement.

The new headquarters will provide approximately 3 times as much office space as does the old residence that the Association now occupies at 1515 Massachusetts Avenue. Such an increase should prove ample for as far ahead as we can safely predict our needs. As a matter of fact, for some years there is likely to be space available for one or two affiliated societies that want to move in with us. A ground-floor auditorium with a capacity of about 200 is a possibility but not yet a certainty.

The building will be modern in design and construction. The movable louvers shown across the front of the second and third floors constitute one of its most unique features. They are designed to shield the large glass areas from direct sunlight and will reduce substantially the cost of air-conditioning the building.

The Association's appearance before the Board of Zoning Adjustment on 30 July was its second attempt to secure permission to erect a headquarters building. An earlier effort in 1952 was denied. In making a second attempt, the Association changed its plans in a number of respects. Most important was the reduction in size from a building of eight stories to one of three. The larger building, planned to accommodate a number of affiliated societies as well as the AAAS, was too much for the Board of Zoning Adjustment to approve for construction in a residential area. The smaller building won the unanimous approval and will be adequate for the AAAS but may be a disappointment to those who had hoped that we could bring a number of scientific societies together under the same roof.



It will take several months to complete final plans and to make arrangements with a building contractor. Work can be expected to start in the early spring of 1955, and the building should be ready for occupancy approximately a year later. We can look forward, in 1956, to moving into our permanent headquarters, a building of our own design, and one that the Association has been talking about ever since the site was purchased in 1946.

DAEL WOLFLE

Administrative Secretary

MEETING OF THE SOUTHWESTERN AND ROCKY MOUNTAIN DIVISION

The 30th annual meeting of the Southwestern Division of the AAAS was held 25–29 Apr. at Texas Technological College, Lubbock. This was the third time that the Association had enjoyed the hospitality of Lubbock and the college, the earlier occasions having been in 1934 and 1941. Those of us who had the good fortune to be at all three meetings could not help but marvel at the phenomenal growth of both the city and the college during these 20 years.

Contributing to the success of the meeting was the attendance of the Southwestern Association of Naturalists (SWAN). Although organized only a little more than a year ago, it has already become an important factor in fostering scientific progress in the region. Other cooperating societies were the Texas and Oklahoma academies of science, the Texas Archaeological Society, and the Panhandle Plains Section of the American Chemical Society.

Under the auspices of the Society of Sigma Xi, Henry Eyring, dean of the graduate school of the University of Utah, delivered an after-dinner speech on "The physical chemistry of enzymes in luminescent bacteria and nerves." Eyring spoke in such a manner that both scientists and laymen enjoyed and profited from his discussion. Roger J. Williams, biochemist of the University of Texas, delivered the annual John Wesley Powell lecture, choosing as his topic, "A new era in human understanding."

Four symposiums dealt with "Instrumentation," "Ground water of the south plains of Texas," "Natural history of the Lubbock area," and "Geology and oil technology." An idea of the wealth of material discussed can be gleaned by noting such titles as "Radiological instrumentation," "Application of transducers in the measurement of physical quantities," "Underground water flow," "The use of fish embryos in biological assay tests," "Differentiation in call among southwestern anuran amphibians," "The interpretation of seismic data obtained in geophysical prospecting," "The internal plastic coating of pipelines in place," "The use of emulsions to stimulate petroleum production," and "Radiotracer survey as a gas and oil well service."

Eight social science papers included such titles as "Some repeated pictograph forms in the Big Bend of Texas," "Acculturation of the Navajo In-

dians," and "Human sterilization laws and psychiatric thinking." Among the 23 papers on botanical subjects, there were such titles as "Successful relations of plant communities of the Elk Mountain Range," "The effect of certain soil correctives on nutrition of seedlings," "Evolution of college botany teaching," "Ladies botany," "The absorption of 2, 4-D through the root systems of 60 woody plants," and "A study of some factors affecting the quality of cottage cheese."

There were 11 papers in the field of zoology, and among them were included "Birds of the south plains," "The nasal mites of finches," "Predicting the variation of fixatives for cells at various altitudes," "The effect of certain chemicals on the eggs and larvae of the dog hookworm," and "Inhibition of antidiuretic activity of blood serum with adrenal medullary hormones." Among the 31 physical science papers, there were such titles as "Parallax in mathematics," "Influence of planetary alignment cycles on climatic variation," "Ion exchange in clays," "A spectrographic study of the venoms of some poisonous desert animals," "The effects of ionizing radiations on the chemical composition and carcinogenicity of organic and biochemical compounds," "A polarographic study of the zirconium—alizarin lake," "The motion of a conducting sphere in a uniform magnetic field," "Elastic scattering of fast neutrons," and "The fluorescence of alpha naphthylphenyl oxazole in various solvents."

Ever since the Southwestern Division was organized in 1920, a great deal of its interest has been centered on problems peculiar to arid lands. In the beginning, this took the form of archeological studies of the prehistoric peoples who once lived in the region. The very dry climate had been favorable to the preservation of many ruins, which, in more humid regions, would have disintegrated more easily and have been covered with vegetation. Since many timbers in these centuries-old buildings were still intact, the idea of studying the tree rings of the logs in the ruins was conceived. As a result of these studies, it was found that perfect correlation existed in the patterns of the tree rings so that the old buildings could be dated with a high degree of accuracy.

Since the thickness of tree-ring bands has been found to be directly related to rainfall, and to some extent to sunspots, a weather pattern and weather cycles could be studied in the hope of some day being able to make long-range weather predictions. Desert plants have been studied with respect to their chemical make-up and possible commercial use, and desert animals, such as gila monsters and rattlesnakes, have been studied with regard to the use of venoms and antivenoms.

Several years ago the Division set up a special committee on desert and arid zone research. Through its efforts, the UNESCO committee on arid lands has agreed to work with the Southwestern Division on the organization of an international symposium to be held jointly with the 24–28 Apr. 1955 meeting of the Division in Santa Fe and Albuquerque, N.M. It is also possible that the regular sessions will be followed by a workshop session for a week to 10 days at Socorro under the auspices of the New Mexico Institute of Mining and Technology. These sessions would be devoted primarily to weather and rain making.

Some 300 persons registered for the 1954 meeting, many coming from states well beyond the limits of the Division. Because Wyoming and part of Montana had recently joined the Division, it was voted to recommend to the national office that the name be changed to *Southwestern and Rocky Mountain Division*. Joe Dennis of Texas Technological College and E. F. Castetter of the University of New Mexico were elected president and vice president, respectively. Frank E. E. Germann of the University of Colorado was continued as secretary-treasurer.

FRANK E. E. GERMANN
University of Colorado, Boulder

Grandeur consists in form, and not in size; and to the eye of the philosopher, the curve drawn on a paper two inches long is just as magnificent, just as symbolic of divine mysteries and melodies, as when embodied in the span of some cathedral roof.—CHARLES KINGSLEY.

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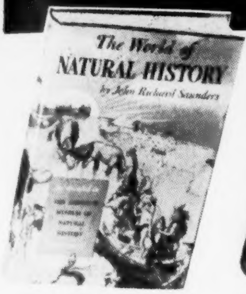

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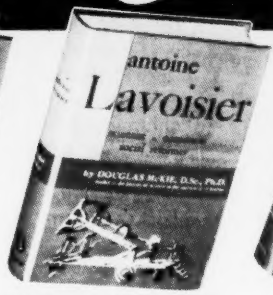

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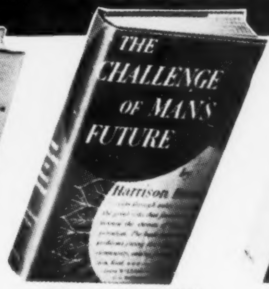

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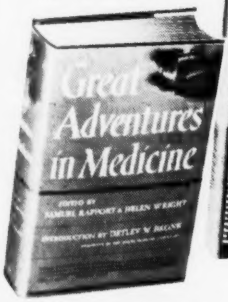

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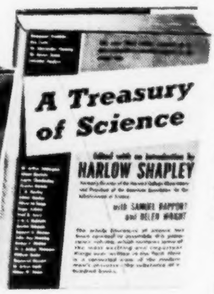

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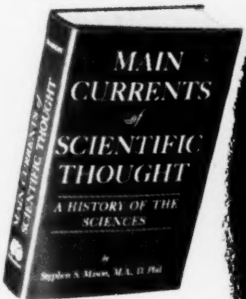

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Meetings

October

19. American Soc. of Safety Engineers, Chicago. (J. B. Johnson, 425 N. Michigan Ave., Chicago 11.)

21–22. National Noise Abatement Symposium, 5th annual, Chicago, Ill. (S. M. Potter, Illinois Inst. of Technology, Chicago 16.)

21–23. International Assoc. of Milk and Food Sanitarians, Atlantic City, N. J. (H. L. Thommasson, IAMFS, Box 437, Shelbyville, Ind.)

21–23. International Symposium on the Dynamics of Virus Infections, Detroit, Mich. (Henry Ford Hospital, 2799 W. Grand Blvd., Detroit 2.)

21–24. American Dietetic Assoc., annual, Philadelphia. (E. A. Atkinson, 620 N. Michigan Ave., Chicago 11.)

22–26. Symposium on Wind and Solar Energy, New Delhi, India. (UNESCO, 19 Ave. Kléber, Paris 16.)

24–27. Soc. of American Foresters, Milwaukee. (H. Clepper, 425 Mills Bldg., Washington 6, D. C.)

25–27. Assoc. of American Medical Colleges, Bedford Springs, Pa. (AAMC, 5 S. Wabash Ave., Chicago 3.)

25–30. International Cong. of Odontology, 1st, São Paulo, Brazil. (F. Degni, Rua Marconi 131, São Paulo.)

26. Assoc. of Consulting Chemists and Chemical Engineers, annual, New York City. (A. B. Bowers, 50 E. 41 St., New York 17.)

27–29. International Symposium on the Hypophyseal Growth Hormone. Its Nature and Actions, Detroit, Mich. (R. W. Smith, Henry Ford Hospital, Detroit 2.)

28–30. American Soc. for Aesthetics, Bloomington, Ind. (R. R. Patrick, Cleveland Museum of Art, Cleveland 6, Ohio.)

28–30. International Symposium on Temperature, Washington, D. C. (W. Waterfall, 57 E. 55 St., New York 22.)

31–1. American Soc. for the Study of Arteriosclerosis, annual, Chicago. (A. C. Corcoran, Cleveland Clinic, Cleveland 6, Ohio.)

November

1–3. Entomological Societies of Canada and Ontario, annual. Sault Ste. Marie, Canada. (R. H. Wigmore, Science Service Bldg., Ottawa.)

1–3. Geological Soc. of America, annual, Los Angeles, Calif. (H. R. Aldrich, 419 W. 117 St., New York 27.)

1–3. Paleontological Soc., annual, Los Angeles, Calif. (K. E. Caster, Dept. of Geology, Univ. of Cincinnati, Cincinnati 21, O.)

1–5. National Metal Cong., Chicago, Ill. (C. L. Wells, 7301 Euclid Ave., Cleveland, O.)

3–5. American Crystallographic Assoc., and 12th annual Pittsburgh Diffraction Cong., Pittsburgh, Pa. (P. K. Koh, Allegheny Ludlum Research Laboratories, Alabama Ave., Brackenridge, Pa.)

3–5. Soc. of Rheology, Washington, D.C. (W. R. Willets. Titanium Pigment Corp., 99 Hudson St., New York 13.)

3–6. American Soc. of Parasitologists, Memphis, Tenn. (A. C. Walton, Dept. of Biology, Knox College, Galesberg, Ill.)

3–6. American Soc. of Tropical Medicine and Hygiene, Memphis, Tenn. (J. E. Larsh, Jr., School of Public Health, Univ. of North Carolina, Chapel Hill.)

4–5. Hawaiian Acad. of Science, Honolulu, Hawaii. (… C. Cox, Experiment Station, HSPA, Honolulu 14.)

4–6. American Acad. of Tropical Medicine, Memphis, Tenn. (E. H. Hinman, Univ. of Puerto Rico School of Medicine, San Juan 22.)

6. Committee for the Scientific Study of Religion, fall, Cambridge, Mass. (R. V. McCann, 48 Mt. Auburn St., Cambridge.)

7. American College of Dentists, Miami, Fla. (O. W. Brandhorst, 4221 Lindell Blvd., St. Louis, Mo.)

7–12. International Cong. of Military Medicine and Pharmacy, Luxemburg, Luxembourg. (A. R. Verneng… Pozos 2045, Buenos Aires, Argentina.)

8–11. American Dental Assoc., annual, Miami, Fla. (… Hillenbrand, 222 E. Superior St., Chicago 11, Ill.)

8–11. American Petroleum Inst., 34th annual, Chicago, Ill. (API, 50 W. 50 St., New York 20.)

8–11. Southern Soc. of Cancer Cytology, annual, St. Louis, Mo. (J. E. Ayre, 1155 N. W. 14 St., Miami, Fla.)

8–12. American Soc. of Agronomy, annual, St. Paul, Minn. (L. G. Monthey, 2702 Monroe St., Madison … Wis.)

8–12. Soil Conservation Soc. of America, St. Paul, Minn. (H. W. Pritchard, 1016 Paramount Bldg., Des Moines, Iowa.)

10–11. Conf. on Electrical Techniques in Medicine and Biology, Chicago, Ill. (E. D. Trout, 4855 Electric Ave., Milwaukee, Wis.)

11–12. Corrosion Cong., Frankfurt, Germany. (DECHEMA) Frankfurt a.M., W. 13.)

12–13. Inter-Society Cytology Council, Boston, Mass. (P. F. Fletcher, 634 N. Grand Blvd., St. Louis 3, Mo.)

12–Dec. 11. United Nations Educational, Scientific and Cultural Organization, Montevideo, Uruguay. (UNESCO, 19 Ave. Kléber, Paris 16.)

15–17. National Conf. on Standards, 5th, New York City. (D. E. Denton, 70 E. 45 St., New York 17).

17–19. American Meteorological Soc., Miami Beach, Fla. (K. C. Spengler, 3 Joy St., Boston 8, Mass.)

18–20. Acoustical Soc. of America, semiannual, Austin, Tex. (W. Waterfall, 57 E. 55 St., New York 22.)

19–30. International Cong. of Civil Engineers, 2nd, Caracas, Venezuela. (L. B. Diaz, Av. Principal de Los Caobos, Apartado 2006, Caracas.)

26–27. American Physical Soc., Chicago, Ill. (K. K. Darrow, Columbia Univ., New York 27.)

26–27. American Soc. of Animal Production, Chicago, Ill. (W. M. Beeson, Dept. of Animal Husbandry, Purdue Univ., Lafayette, Ind.)

26–27. Tennessee Acad. of Science, Nashville, Tenn. (I. H. Tipton, Physics Dept., Univ. of Tennessee, Knoxville.)

28–1. American Soc. of Refrigerating Engineers, Philadelphia, Pa. (J. I. Szabo, 40 W. 40 St., New York 18.)

28–3. American Soc. of Mechanical Engineers, annual, New York City. (O. B. Schier, II, 29 W. 39 St., New York 18.)

29–1. Assoc. of Military Surgeons, Washington, D. C. (R. R. Sayers, Armed Forces Inst. of Pathology, Washington 25.)

29–2. American Medical Assoc., clinical, Miami, Fla. (… F. Lull, 535 Dearborn St., Chicago 10, Ill.)